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PROCEEDINGS

OF

THE AMERICAN ASSOCIATION

FOR THE

ADVANCEMENT OF SCIENCE.

SEVENTEENTH MEETING,

HELD AT

CHICAGO, ILLINOIS,

AUGUST, 1868.

BOSTON MEDICAL LIBRARY ASS'N.
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CAMBRIDGE:
PUBLISHED BY JOSEPH LOVERING.
1869.

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EDITED BY

JOSEPH LOVERING,

Permanent Secretary.

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JUL 18 1892
E. H. B.

SALEM:
Essex Institute Press.

TABLE OF CONTENTS.

COMMUNICATIONS.

A. MATHEMATICS, PHYSICS, AND CHEMISTRY.

B. NATURAL HISTORY.

EXECUTIVE PROCEEDINGS.

OFFICERS OF THE ASSOCIATION

AT

THE CHICAGO MEETING.

B. A. GOULD, *President.*
CHARLES WHITTLESEY, *Vice-President.*
JOSEPH LOVERING, *Permanent Secretary.*
A. P. ROCKWELL,* *General Secretary.*
A. L. ELWYN, *Treasurer.*

Standing Committee.

EX-OFFICIO.

B. A. GOULD,	J. S. NEWBERRY,
CHARLES WHITTLESEY,	WOLCOTT GIBBS,
JOSEPH LOVERING,	C. S. LYMAN,
A. P. ROCKWELL,*	A. L. ELWYN.

AS CHAIRMEN OF THE SECTIONAL COMMITTEES.

WILLIAM CHAUVENET,	J. D. WHITNEY.

FROM THE ASSOCIATION AT LARGE.

F. A. P. BARNARD,	J. H. COFFIN,
H. L. EUSTIS,	G. W. HOUGH,
H. A. NEWTON,	J. H. RAUCH,
C. C. PARRY,	S. D. TILLMAN.

* As Mr. ROCKWELL was suddenly called home on the first day of the meeting Mr. SIMON NEWCOMB was appointed to fill his place.

Local Committee.

J. YOUNG SCAMMON, *Chairman.*
WILLIAM STIMPSON, *Secretary.*

EDMUND ANDREWS,
J. F. BEATY,
J. V. Z. BLANEY,
E. W. BLATCHFORD,
WILLIAM BROSS,
E. S. CHESBROUGH,
EDWARD DANIELS,
W. E. DOGGETT,
J. W. FOSTER,
DAVID A. GAGE,
WALTER HAY,
E. B. MCCAGG,
JOHN H. RAUCH,
CHARLES H. REED,
J. B. RICE,
T. H. SAFFORD,
E. H. SHELDON,
PERRY H. SMITH,
DANIEL THOMPSON,
GEORGE C. WALKER,
C. L. WILSON,
JOHN M. WOODWORTH.

Local Sub-Committees.

On Reception—Messrs. RAUCH, SAFFORD, MCCAGG, RICE, and BROSS.

On Lodging and Entertainment—Messrs. SCAMMON, MCCAGG, DOGGETT, BROSS, THOMPSON, GAGE and HAY.

On Rooms—Messrs. BLANEY, WALKER and ANDREWS.

On Finance—Messrs. DOGGETT, WALKER, BEATY and BROSS.

On Excursions—Messrs. DANIELS, SHELDON, BEATY, BLATCHFORD, WILSON AND SMITH.

On Invitation, Correspondence and Printing—Messrs. HAY, WOODWORTH and STIMPSON.

On Railroads—Messrs. CHESBROUGH, FOSTER, SHELDON and BLANEY.

SPECIAL COMMITTEES.

A. Committees continued from former Meetings.

1. *Committee to Report in Relation to Uniform Standards in Weights, Measures, and Coinage.*

F. A. P. Barnard,
John F. Frazer,
J. H. Gibbon,
Wolcott Gibbs,
B. A. Gould,
Joseph Henry,
J. E. Hilgard,
John LeConte,
H. A. Newton,
Benjamin Peirce,
W. B. Rogers,
J. L. Smith,
John Torrey.

B. New Committees.

2. *Committee to Audit the Accounts of the Permanent Secretary and the Treasurer.*

B. A. Gould,
H. L. Eustis,

3. *Committee with whom the Permanent Secretary may advise in regard to the publication of the Chicago Proceedings.*

H. A. Newton,
J. S. Newberry.

4. *Committee to act with the Standing Committee in nomination of Officers for the Salem Meeting.*

Section A.
Elias Loomis,
Cleveland Abbe,
J. C. Watson,
G. W. Hough,

Section B.
E. A. Dalrymple,
O. C. Marsh,
C. A. White,
J. W. Foster.

5. *Committee to address Dr. Ehrenberg on the occasion of his approaching Jubilæum.*

Louis Agassiz,
F. A. P. Barnard,
James D. Dana,
B. A. Gould,
Joseph Lovering,
W. S. Sullivant,
John Torrey.

6. *Committee to whom was referred Prof. Benjamin Peirce's communication relating to the changes in the star Eta Argus.*

B. A. Gould,
James C. Watson.
Simon Newcomb.

OFFICERS OF THE SALEM MEETING.

J. W. FOSTER, *President.*
O. N. ROOD, *Vice-President.*
JOSEPH LOVERING, *Permanent Secretary.*
O. C. MARSH, *General Secretary.*
A. L. ELWYN, *Treasurer.*

Standing Committee.

J. W. FOSTER,
O. N. ROOD,
JOSEPH LOVERING,
O. C. MARSH,
B. A. GOULD,
CHARLES WHITTLESEY,
A. P. ROCKWELL,
A. L. ELWYN.

Local Committee.

HENRY WHEATLAND, *Chairman.*
F. W. PUTNAM, *Secretary.*
WILLIAM SUTTON, *Treasurer.*

His Honor, WILLIAM COGGSWELL, *Mayor of Salem.*
C. S. OSGOOD, *President of the Common Council.*

E. S. ATWOOD,
J. H. BATCHELDER,
J. BERTRAM,
E. BICKNELL,
R. BROOKHOUSE,
C. COOKE,
B. COX,
A. CROSBY,
A. W. DODGE,
W. C. ENDICOTT,
A. C. GOODELL, Jr.,
L. B. HARRINGTON,
N. A. HORTON,
A. HUNTINGTON,
A. HYATT,
A. H. JOHNSON,
J. KIMBALL,
H. F. KING,
J. C. LEE,
G. B. LORING,
E. S. MORSE,
J. C. OSGOOD,
A. S. PACKARD, Jr.,
C. W. PALFRAY,
C. R. PALMER,
S. E. PEABODY,
G. PERKINS,
G. A. PERKINS,
W. P. PHILLIPS,
G. D. PHIPPEN,
C. H. PRICE,
R. S. RANTOUL,
J. ROBINSON,
C. A. ROPES,
G. P. RUSSELL,
B. H. SILSBEE,
A. A. SMITH,
E. SUTTON,
J. C. TOWNE,
W. P. UPHAM,
R. P. WATERS,
B. WEBB, Jr.,
B. A. WEST,
S. G. WHEATLAND,
H. L. WILLIAMS,
E. B. WILLSON.

MEETINGS OF THE AMERICAN ASSOCIATION FOR THE ADVANCEMENT OF SCIENCE.

Meeting.	Date.	Place.	President.	Vice-President.	General Secretary.	Perman't Secretary.	Treasurer.
1st,	Sept. 20, 1848,	Philadelphia, Pa.,	W. C. Redfield,		Walter R. Johnson,		Jeffries Wyman,
2d,	Aug. 14, 1849,	Cambridge, Mass.,	Joseph Henry,		E. N. Horsford,		A. L. Elwyn,
3d,	March 12, 1850,	Charleston, S. C.,	A. D. Bache,*		L. R. Gibbs,*		St. J. Ravenel.*
4th,	Aug. 19, 1850,	New Haven, Ct.,	A. D. Bache,		E. C. Herrick,		A. L. Elwyn.
5th,	May 5, 1851,	Cincinnati, Ohio,	A. D. Bache,		W. B. Rogers,	S. F. Baird,	A. L. Elwyn.
6th,	Aug. 19, 1851,	Albany, N. Y.,	Louis Agassiz,		W. B. Rogers,	S. F. Baird,	A. L. Elwyn.
7th,	July 28, 1853,	Cleveland, Ohio,	Benj. Peirce,		J. D. Dana,	S. F. Baird,	A. L. Elwyn.
8th,	April 26, 1854,	Washington, D. C.,	J. D. Dana,		J. Lawrence Smith,	Joseph Lovering,	J. L. LeConte.*
9th,	Aug. 15, 1855,	Providence, R. I.,	John Torrey,		Wolcott Gibbs,	Joseph Lovering,	A. L. Elwyn.
10th,	Aug. 20, 1856,	Albany, N. Y.,	James Hall,		B. A. Gould,	Joseph Lovering,	A. L. Elwyn.
11th,	Aug. 12, 1857,	Montreal, C. E.,	J. W. Bailey,	Alexis Caswell,	John LeConte,	Joseph Lovering,	A. L. Elwyn.
12th,	April 28, 1858,	Baltimore, M. D.,	Alexis Caswell,*	John E. Holbrook,	W. M. Gillespie,	Joseph Lovering,	A. L. Elwyn.
13th,	Aug. 3, 1859,	Springfield, Mass.,	S. Alexander,	Edward Hitchcock,	William Chauvenet,	Joseph Lovering,	A. L. Elwyn.
14th,	Aug. 1, 1860,	Newport, R. I.,	Isaac Lea,	B. A. Gould,	Joseph LeConte,	Joseph Lovering,	A. L. Elwyn.
15th,	Aug. 15, 1866,	Buffalo, N. Y.,	F. A. P. Barnard,	A. A. Gould,	Elias Loomis,	Joseph Lovering,	A. L. Elwyn.
16th,	Aug. 21, 1867,	Burlington, Vt.,	J. S. Newberry,	Wolcott Gibbs,	C. S. Lyman,	Joseph Lovering,	A. L. Elwyn.
17th,	Aug. 5, 1868,	Chicago, Ill.,	B. A. Gould,	Charles Whittlesey,	Simon Newcomb,*	Joseph Lovering,	A. L. Elwyn.

* In the absence of the regular officer.

CONSTITUTION OF THE ASSOCIATION.*

OBJECTS.

THE Association shall be called THE AMERICAN ASSOCIATION FOR THE ADVANCEMENT OF SCIENCE.

The objects of the Association are, by periodical and migratory meetings, to promote intercourse between those who are cultivating science in different parts of the United States, to give a stronger and more general impulse and a more systematic direction to scientific research in our country, and to procure for the labors of scientific men increased facilities and a wider usefulness.

MEMBERS.

RULE 1. Any person may become a member of the Association upon recommendation in writing by two members, nomination by the Standing Committee, and election by a majority of the members present.

OFFICERS.

RULE 2. The officers of the Association shall be a President, Vice-President, General Secretary, Permanent Secretary, and Treasurer. The President, Vice-President, General Secretary, and Treasurer shall be elected at each meeting for the following one;—the three first named officers not to be reëligible for the next two meetings, and the Treasurer to be reëligible as long as the Association may desire. The Permanent Secretary shall be elected at each second meeting, and also be reëligible as long as the Association may desire.

* Adopted August 25, 1856, and ordered to go into effect at the opening of the Montreal Meeting. Amended at Burlington, August, 1867, and at Chicago, August, 1868.

MEETINGS.

Rule 3. The Association shall meet, at such intervals as it may determine, for one week, or longer, — the time and place of each meeting being determined by a vote of the Association at the previous meeting; and the arrangements for it shall be intrusted to the officers and the Local Committee.

STANDING COMMITTEE.

Rule 4. There shall be a Standing Committee, to consist of the President, Vice-President, Secretaries, and Treasurer of the Association, the officers of the preceding year, the permanent Chairman of the Sectional Committees, after these shall have been organized, and six members present from the Association at large, who shall have attended any of the previous meetings, to be elected upon open nomination by ballot on the first assembling of the Association. A majority of the whole number of votes cast, to elect. The General Secretary shall be Secretary of the Standing Committee.

The duties of the Standing Committee shall be, —

1. To assign papers to the respective sections.

2. To arrange the scientific business of the general meetings, to suggest topics, and arrange the programmes for the evening meetings.

3. To suggest to the Association the place and time of the next meeting.

4. To examine, and, if necessary, to exclude papers.

5. To suggest to the Association subjects for scientific reports and researches.

6. To appoint the Local Committee.

7. To have the general direction of publications.

8. To manage any other general business of the Association during the session, and during the interval between it and the next meeting.

9. In conjunction with four from each Section, to be elected by the Sections for the purpose, to make nominations of officers of the Association for the following meeting.

10. To nominate persons for admission to membership.

11. Before adjourning, to decide which papers, discussions, or other proceedings shall be published.

SECTIONS.

RULE 5. The Association shall be divided into two Sections, and as many sub-sections as may be necessary for the scientific business, the manner of division to be determined by the Standing Committee of the Association. The two Sections may meet as one.

SECTIONAL OFFICERS AND COMMITTEES.

RULE 6. On the first assembling of the Section, the members shall elect upon open nomination a permanent Chairman and Secretary, also three other members, to constitute, with these officers, a Sectional Committee.

The Section shall appoint, from day to day, a Chairman to preside over its meetings.

RULE 7. It shall be the duty of the Sectional Committee of each Section to arrange and direct the proceedings in their Section; to ascertain what communications are offered; to assign the order in which these communications shall appear, and the amount of time which each shall occupy.

The Sectional Committees may likewise recommend subjects for systematic investigation by members willing to undertake the researches, and to present their results at the next meeting.

The Sectional Committee may likewise recommend reports on particular topics and departments of science, to be drawn up as occasion permits, by competent persons, and presented at subsequent meetings.

REPORTS OF PROCEEDINGS.

RULE 8. Whenever practicable the proceedings shall be reported by professional reporters, or stenographers, whose reports are to be revised by the Secretaries before they appear in print.

PAPERS AND COMMUNICATIONS.

RULE 9. No paper shall be placed in the programme, unless admitted by the Sectional Committee; nor shall any be read, unless an abstract of it has been previously presented to the Secretary of the Section, who shall furnish to the Chairman the titles of papers, of which abstracts have been received.

RULE 10. The author of any paper or communication shall be at liberty to retain his right of property therein, provided he declares such to be his wish before presenting it to the Association.

RULE 11. Copies of all communications, made either to the General Association or to the Sections, must be furnished by the authors; otherwise only the titles, or abstracts, shall appear in the published proceedings.

RULE 12. All papers, either at the general or in the sectional meetings, shall be read, as far as practicable, in the order in which they are entered upon the books of the Association; expect that those which may be entered by a member of the Standing Committee of the Association shall be liable to postponement by the proper Sectional Committee.

RULE 13. If any communication be not ready at the assigned time, it shall be dropped to the bottom of the list, and shall not be entitled to take precedence of any subsequent communication.

RULE 14. No exchanges shall be made between members without authority of the respective Sectional Committees.

GENERAL AND EVENING MEETINGS.

RULE 15. The Standing Committee shall appoint any general meeting which the objects and interests of the Association may call for, and the evenings shall, as a rule, be reserved for general meetings of the Association.

These general meetings may, when convened for that purpose, give their attention to any topics of science which would otherwise come before the Sections.

It shall be a part of the business of these general meetings to receive the Address of the President of the last meeting; to hear such reports on scientific subjects as, from their general importance and interests, the Standing Committee shall select; also to receive from the Chairman of the Sections, abstracts of the proceedings of their respective Sections; and to listen to communications and lectures explanatory of new and important discoveries and researches in science, and new inventions and processes in the arts.

ORDER OF PROCEEDINGS IN ORGANIZING A MEETING.

RULE 16. The Association shall be called to order by the President of the preceding meeting; and this officer having resigned the chair to the President elect, the General Secretary shall then report the number of papers relating to each department which have been registered, and the Association consider the most eligible distribution into Sections, when it shall proceed to the election of the additional members of the Standing Committee in the manner before described; the meeting shall then adjourn, and the Standing Committee, having divided the Association into sections as directed, shall allot to each its place of meeting for the Session. The Sections shall then organize by electing their officers and their representatives in the Nominating Committee, and shall proceed to business.

PERMANENT SECRETARY.

RULE 17. It shall be the duty of the Permanent Secretary to notify members who are in arrears, to provide the necessary stationery and suitable books for the list of members and titles of papers, minutes of the general and sectional meetings, and for other purposes indicated in the rules, and to execute such other duties as may be directed by the Standing Committee or by the Association.

The Permanent Secretary shall make a report annually to the Standing Committee, at its first meeting, to be laid before the Association, of the business of which he has had charge since its last meeting.

All members are particularly desired to forward to the Permanent Secretary, so as to be received before the day appointed for the Association to convene, complete titles of all the papers which they expect to present during its meeting, with an estimate of the time required for reading each, and such abstracts of their contents as may give a general idea of their nature.

Whenever the Permanent Secretary notices any error of fact or unnecessary repetition, or any other important defect in the papers communicated for publication in the proceedings of the Association, he is authorized to commit the same to the author, or to the proper subcommittee of the Standing Committee for correction.

LOCAL COMMITTEE.

RULE 18. The Local Committee shall be appointed from among members residing at, or near, the place of meeting for the ensuing year; and it shall be the duty of the Local Committee, assisted by the officers, to make arrangements and the necessary announcements for the meeting.

The Secretary of the Local Committee shall issue a circular in regard to the time and place of meetings, and other particulars, at least one month before each meeting.

SUBSCRIPTIONS.

RULE 19. The amount of the subscription, at each meeting, of each member of the Association, shall be two dollars, and one dollar in addition shall entitle him to a copy of the proceedings of the annual meeting. These subscriptions shall be received by the Permanent Secretary, who shall pay them over, after the meeting, to the Treasurer.

The admission fee of new members shall be five dollars, in addition to the annual subscription: and no person shall be considered a member of the Association until this admission fee and the subscription for the meeting at which he is elected have been paid.

RULE 20. The names of all persons two years in arrears for annual dues shall be erased from the list of members; provided that two notices of indebtedness, at an interval of at least three months, shall have been previously given.

ACCOUNTS.

RULE 21. The accounts of the Association shall be audited, annually, by auditors appointed at each meeting.

ALTERATIONS OF THE CONSTITUTION.

RULE 22. No article of this constitution shall be altered, or amended, or set aside, without the concurrence of three-fourths of the members present, and unless notice of the proposed change shall have been given at the preceding annual meeting.

RESOLUTIONS

OF A PERMANENT AND PROSPECTIVE CHARACTER, ADOPTED AUGUST 19, 1857.

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1. No appointment may be made in behalf of the Association, and no invitation given or accepted, except by vote of the Association or its Standing Committee.

2. The General Secretary shall transmit to the Permanent Secretary for the files, within two weeks after the adjournment of every meeting, a record of the proceedings of the Association and the votes of the Standing Committee. He shall also, daily, during the meetings, provide the Chairman of the two Sectional Committees with lists of the papers assigned to their Sections by the Standing Committee.

3. All printing for the Association shall be superintended by the Permanent Secretary, who is authorized to employ a clerk for that especial purpose.

4. The Permanent Secretary is authorized to put the proceedings of the meetings to press one month after the adjournment of the Association. Papers which have not been received at that time may be published only by title. No notice of articles not approved shall be taken in the published proceedings.

5. The Permanent Chairmen of the Sections are to be considered their organs of communication with the Standing Committee.

6. It shall be the duty of the Secretaries of the two Sections to receive copies of the papers read in their Sections, all subsections included, and to furnish them to the Permanent Secretary at the close of the meeting.

7. The Sectional Committees shall meet not later than 9 A.M. daily, during the meetings of the Association, to arrange

the programmes of their respective sections, including all subsections, for the following day. No paper shall be placed upon these programmes which shall not have been assigned to the Section by the Standing Committee. The programmes are to be furnished to the Permanent Secretary not later than 11 A.M.

8. During the meetings of the Association, the Standing Committee shall meet daily, Sundays excepted, at 9 A.M., and the Sections be called to order at 10 A.M., unless otherwise ordered. The Standing Committee shall also meet on the evening preceding the first assembling of the Association at each annual meeting, to arrange for the business of the first day; and on this occasion three shall form a quorum.

9. Associate members may be admitted for one, two, or three years as they shall choose at the time of admission,—to be elected in the same way as permanent members, and to pay the same dues. They shall have all the social and scientific privileges of members, without taking part in the business.

10. No member may take part in the organization and business arrangement of both the Sections.

MEMBERS

OF THE

AMERICAN ASSOCIATION

FOR THE

ADVANCEMENT OF SCIENCE.

A.

Abbe, Cleveland, Cincinnati, Ohio (16).
*Adams, C. B., Amherst, Massachusetts (1).
Adelberg, Justus, New York, New York (15).
Aiken, W. E. A., Baltimore, Maryland (12).
Ainsworth, J. G., Barry, Massachusetts (14).
Albert, Augustus J., Baltimore, Maryland (12).
Alexander, Stephen, Princeton, New Jersey (1).
Allen, Zachariah, Providence, Rhode Island (1).
*Ames, M. P., Springfield, Massachusetts (1).
Andrews, E. B., Marietta, Ohio (7).
Angell, James B., Burlington, Vermont (16).
*Appleton, Nathan, Boston, Massachusetts (1).

B.

*Bache, Alexander D., Washington, District of Columbia (1).
Bacon, John, Jr., Boston, Massachusetts (1).
*Bailey, J. W., West Point, New York (1).
Baird, S. F., Washington, District of Columbia (1).
Bardwell, F. W., Jacksonville, Florida (13).
Barker, G. F., New Haven, Connecticut (13).
Barlow, Thomas, Canastota, New York (7).
Barnard, F. A. P., New York, New York (7).
Barnard, Henry, Madison, Wisconsin (12).
Barnard, J. G., Washington, District of Columbia (14).
Barnes, James, Springfield, Massachusetts (5).

NOTE.—Names of deceased members are marked with an asterisk [*]. The figure at the end of each name refers to the meeting at which the election took place.

Barratt, Joseph, Middletown, Connecticut (13).
Basnett, Thomas, Ottawa, Illinois (8).
Batchelder, J. M., Cambridge, Massachusetts (8).
Beadle, E. R., Hartford, Connnecticut (10).
* Beck, C. F., Philadelphia, Pennsylvania (1).
* Beck, Lewis C., New Brunswick, New Jersey (1).
* Beck, T. Romeyn, Albany, New York (1).
Bell, Samuel N., Manchester, New Hampshire (7).
Benedict, G. W., Burlington, Vermont (16).
Bigelow, George H., Burlington, Vermont (16).
* Binney, Amos, Boston, Massachusetts (1).
* Binney, John, Boston, Massachusetts (3).
Bird, William A., Buffalo, New York (15).
Blake, Eli W., Ithaca, New York (15).
Blake, Eli W., New Haven, Connecticut (1).
Blake, W. P., San Francisco, California (2).
* Blanding, William, Rhode Island (1).
Blaney, J. Van Zandt, Chicago, Illinois (12).
* Bomford, George, Washington, District of Columbia (1).
Bouvé, Thomas T., Boston, Massachusetts (1).
Bowditch, Henry I., Boston, Massachusetts (2).
Bradford, Isaac, Jacksonville, Florida (14).
Bradish, Alvah, Fredonia, New York (15).
Bradley, L., Jersey City, New Jersey (15).
Brayley, James, Buffalo, New York (15).
Brevoort, J. Carson, Brooklyn, New York (1).
Briggs, A. D., Springfield, Massachusetts (13).
Bross, William, Chicago, Illinois (7).
Brown, Robert, Jr., Cincinnati, Ohio (11).
Brush, George J., New Haven, Connecticut (11).
Buchanan, Robert, Cincinnati, Ohio (2).
Buck, C. E., New York, New York (15).
* Burnap, G. W., Baltimore, Maryland (12).
* Burnett, Waldo I., Boston, Massachusetts (1).
Bushee, James, Worcester, Massachusetts (9).
Butler, Thomas B., Norwalk, Connecticut (10).

C.

* Carpenter, Thornton, Camden, South Carolina (7).
* Carpenter, William M., New Orleans, Louisiana (1).
Case, Leonard, Cleveland, Ohio (15).
* Case, William, Cleveland, Ohio (6).
Cassels, J. L., Cleveland, Ohio (7).
Caswell, Alexis, Providence, Rhode Island (2).
Cattell, William C., Easton, Pennsylvania (15).
Chadbourn, P. A., Madison, Wisconsin (10).

Chapin, A. L., Beloit, Wisconsin (14).
* Chapman, N., Philadelphia, Pennsylvania (1).
Chase, George L., Providence, Rhode Island (1).
* Chase, S., Dartmouth, New Hampshire (2).
Chauvenet, William, St. Louis, Missouri (1).
Chesbrough, E. S., Chicago, Illinois (2).
Chester, Albert H., New York, New York (15).
Chester, Albert T., Buffalo, New York (15).
Chittenden, L. E., New York, New York (14).
Churchill, Marlborough, Sing Sing, New York (13).
Clapp, Almon M., Buffalo, New York (15).
* Clapp, Asahel, New Albany, Indiana (1).
* Clark, Joseph, Cincinnati, Ohio (5).
Clark, Henry, Worcester, Massachusetts (14).
Cleaveland, C. H., Cincinnati, Ohio (9).
* Cleveland, A. B., Cambridge, Massachusetts (2).
Clinton, George W., Buffalo, New York (15).
Clum, Henry A., New York, New York (9).
Coakley, George W., New York, New York (5).
Cochran, D. H., Brooklyn, New York (15).
Coffin, C. C., Malden, Massachusetts (13).
Coffin, James H., Easton, Pennsylvania (1).
Coffin, John H. C., Washington, District of Columbia (1).
* Cole, Thomas, Salem, Massachusetts (1).
* Coleman, Henry, Boston, Massachusetts (1).
Comstock, C. B., West Point, New York (14).
Conant, Marshall, Washington, District of Columbia (7).
Conkling, Frederick A., New York, New York (11).
Conway, M. D., Cincinnati, Ohio (14).
Copes, Joseph S., New Orleans, Louisiana (11).
Corning, Erastus, Albany, New York (6).
Craig, B. F., Washington, District of Columbia (15).
Cramp, J. M., Acadia College, Nova Scotia (11).
Credner, Herman, New York, New York (15).
Crosby, Alpheus, Salem, Massachusetts (10).
Cummings, Joseph, Middletown, Connecticut (13).

D.

Dalrymple, E. A., Baltimore, Maryland (11).
Dana, James D., New Haven, Connecticut (1).
Danforth, Edward, Troy, New York (11).
Davis, James, Boston, Massachusetts (1).
Dawson, J. W., Montreal, Canada (10).
Dean, Amos, Albany, New York (6).
Dean, George W., Fall River, Massachusetts (15).
* Dearborn, George H. A. S., Roxbury, Massachusetts (1).

*Dekay, James E., New York, New York (1).
Delano, B. L., Boston, Massachusetts (16).
Delano, Joseph C., New Bedford, Massachusetts (5).
Denson, Claudius B., Pittsborough, North Carolina (12).
*Dewey, Chester, Rochester, New York (1).
Dexter, G. M., Boston, Massachusetts (11).
Dinwiddie, Robert, 113 Water St., New York City, New York (1).
Dixwell, Eps S., Cambridge, Massachusetts (1).
Dobbins, David P., Buffalo, New York (15).
Dorr, E. P., Buffalo, New York (15).
Dow, George W., Chicago, Illinois (13).
Downes, John, Washington, District of Columbia (10).
Drowne, Charles, Troy, New York (6).
*Ducatel, J. T., Baltimore, Maryland (1).
Duffield, George, Detroit, Michigan (10).
*Dumont, A. H., Newport, Rhode Island (14).
*Duncan, Lucius C., New Orleans, Louisiana (10).
*Dunn, R. P., Providence, Rhode Island (14).
Dunn, T. C., Newport, Rhode Island (14).
Dyer, Elisha, Providence, Rhode Island (9).

E.

Easton, Norman, Fall River, Massachusetts (14).
Eaton, Daniel C., New Haven, Connecticut (13).
Eliot, Charles W., Boston, Massachusetts (14).
Elliott, Ezekiel B., Boston, Massachusetts (10).
Elwyn, Alfred L., Philadelphia, Pennsylvania(1).
Emerson, George B., Boston, Massachusetts (1).
Engelmann, George, St. Louis, Missouri (1).
Engstrom, A. B., Burlington, New Jersey (1).
Eustis, Henry L., Cambridge, Massachusetts (2).
Evans, Charles W., Buffalo, New York (15).
*Everett, Edward, Boston, Massachusetts (2).
Everett, J. D., Windsor, Nova Scotia (14).
Ewing, Thomas, Lancaster, Ohio (5).

F.

Fairbanks, Henry, Hanover, New Hampshire (14).
Farnham, Thomas, Buffalo, New York (15).
Ferrell, William, Nashville, Tennessee (11).
Ferris, Isaac, New York, New York (6).
Feuchtwanger, Louis, New York, New York (11).
Field, Roswell, Greenfield, Massachusetts (13).
Fillmore, Millard, Buffalo, New York (7).
Fisher, Mark, Trenton, New Jersey (10),
*Fitch, Alexander, Hartford, Connecticut (1).

Fitch, Edward H., Ashtabula, Ohio (11).
Fitch, O. H., Ashtabula, Ohio (7).
*Forbush, E. B., Buffalo, New York (15).
Force, Peter, Washington, District of Columbia (4).
Fosgate, Blanchard, Auburn, New York (7).
Foster, J. W., Chicago, Illinois (1).
*Fox, Charles, Grosse Isle, Michigan (7).
Frothingham, Frederick, Buffalo, New York (11).

G.

Gay, C. C. F., Buffalo, New York (15).
*Gay, Martin, Boston, Massachusetts (1).
Gibbes, L. R., Charleston, South Carolina (1).
Gibbon, J. H., Charlotte, North Carolina (3).
Gibbs, Walcott, Cambridge, Massachusetts (1).
*Gillespie, W. M., Schenectady, New York (10).
Gillmore, Q. A., Jr., New York, New York (13).
Gilman, Daniel C., New Haven, Connecticut (10).
*Gilmor, Robert, Baltimore, Maryland (1).
Glynn, James, Geneva, New York (1).
Gold, Theodore S., West Cornwall, Connecticut (4).
Goodwin, William F., Richmond, Virginia (10).
*Gould, Augustus A., Boston, Massachusetts (11).
*Gould, B. A., Boston, Massachusetts (2).
Gould, B. A., Cambridge, Massachusetts (2).
*Graham, James D., Washington, District of Columbia (1).
Gray, Asa, Cambridge, Massachusetts (1).
*Gray, James H., Springfield, Massachusetts (6).
Green, Traill, Easton, Pennsylvania (1).
*Greene, Benjamin D., Boston, Massachusetts (1).
Greene, Samuel, Woonsocket, Rhode Island (9).
*Griffith, Robert E., Philadelphia, Pennsylvania (1).
Grinnan, A. G., Orange Court House, Virginia (7).
Grote, Augustus R., Buffalo, New York (15).
Guyot, Arnold, Princeton, New Jersey (1).

H.

*Hackley, Charles W., New York, New York (4).
Hadley, George, Buffalo, New York (6).
Haldeman, S. S., Columbia, Pennsylvania (1).
*Hale, Enoch, Boston, Massachusetts (1).
Hall, James, Albany, New York (1).
Hall, N. K., Buffalo, New York (7).
Hamlin, A. C., Bangor, Maine (10).
Hammond, George T., Newport, Rhode Island (14).
Hance, Ebenezer, Morrisville, Pennsylvania (7).

Hand, Thomas J., Baltimore, Maryland (12).
Hanover, M. D., Cincinnati, Ohio (13).
* Hare, Robert, Philadelphia, Pennsylvania (11).
* Harlan, Joseph G., Haverford, Pennsylvania (8).
* Harlan, Richard, Philadelphia, Pennsylvania (1).
Harman, Henry M., Baltimore, Maryland (12).
* Harris, Thaddeus W., Cambridge, Massachusetts (1).
Harrison, B. F., Wallingford, Connecticut (11).
* Hart, Simeon, Farmington, Connecticut (1).
Hartshorne, Henry, Philadelphia, Pennsylvania (12).
Harvey, Charles W., Buffalo, New York (15).
Harvey, Leon F., Buffalo, New York (15).
Haven, Samuel F., Worcester, Massachusetts (9).
* Hayden, H. H., Baltimore, Maryland (1).
Hayes, George E., Buffalo, New York (15).
* Hayward, James, Boston, Massachusetts (1).
Henry, Joseph, Washington, District of Columbia (1).
Herzer, W., Columbus, Ohio (15).
Hickok, M. J., Scranton, Pennsylvania (11).
Hickok, W. C., Burlington, Vermont (16).
Hilgard, Eugene W., Oxford, Mississippi (11).
Hilgard, Julius E., Washington, District of Columbia (4).
Hill, S. W., Hancock, Lake Superior (6).
Hill, Thomas, Waltham, Massachusetts (8).
Hitchcock, Charles H., Hanover, New Hampshire (11).
* Hitchcock, Edward, Amherst, Massachusetts (1).
Hitchcock, Edward, Amherst, Massachusetts (4).
Hoadley, E. S., Springfield, Massachusetts (13).
Homes, Henry A., Albany, New York (11).
Horsford, E. N., Cambridge, Massachusetts (1).
* Horton, William, Craigville, Orange Co., New York (1).
Hough, Franklin B., Lowville, New York (4).
Hough, G. W., Albany, New York (15).
* Houghton, Douglas, Detroit, Michigan (1).
* Howland, Theodore, Buffalo, New York (15).
Howell, Robert, Nichols, New York (6).
Hubbard, Oliver P., Hanover, New Hampshire (1).
* Hubbert, James, Richmond, Province of Quebec (16).
Hungerford, Edward, Burlington, Vermont (10).
* Hunt, Freeman, New York, New York (11).
Hunt, George, Providence, Rhode Island (9).
* Hunt, E. B., Washington, District of Columbia (2).
Hunt, Sterry, Montreal, Canada (1).
Hyatt, James, Bangall, New York (10).

I.

* Ives, Thomas P., Providence, Rhode Island (10).

J.

Jackson, Charles T., Boston, Massachusetts (1).
Jillson, B. C., Nashville, Tennessee (14).
*Johnson, W. R., Washington, District of Columbia (1).
Johnston, John, Middletown, Connecticut (1).
*Jones, Catesby A. R., Washington, District of Columbia (8).
Joy, C. A., New York, New York (8).
Judd, Orange, New York, New York (4).

K.

Keely, G. W., Waterville, Maine (1).
Keep, N. C., Boston, Massachusetts (13).
Kerr, W. C., Raleigh, North Carolina (10).
Kimball, J. P., New York, New York (15).
King, Mary B. A., Rochester, New York (15).
Kirkpatrick, James A., Philadelphia, Pennsylvania (7).
Kirkwood, Daniel, Canonsburg, Pennsylvania (7).
Kite, Thomas, Cincinnati, Ohio (5).

L.

Lapham, Increase A., Milwaukee, Wisconsin (8).
*Lasel, Edward, Williamstown, Massachusetts (1).
Lattimore, S. A., Lima, New York (15).
Lauderdale, John V., Geneseo, New York (10).
Lawrence, George N., New York, New York (7).
Lea, Isaac, Philadelphia, Pennsylvania (1).
LeConte, John, Columbia, South Carolina (3).
LeConte, John L., Philadelphia, Pennsylvania (1).
LeConte, Joseph, Columbia, South Carolina (8).
*Lederer, Baron von, Washington, District of Columbia (1).
Lee, John R., Buffalo, New York (15).
Leonard, A. M., Lockport, New York (15).
Lesley, Joseph, Jr., Philadelphia, Pennsylvania (8).
Lesley, J. P., Philadelphia, Pennsylvania (2).
Letchworth, William P., Portage, New York (15).
*Lieber, Oscar M., Columbia, South Carolina (8).
*Lincklaen, Ledyard, Cazenovia, New York (1).
Lindsley, J. B., Nashville, Tennessee (1).
*Linsley, James H., Stafford, Connecticut (1).
Little, George, Oxford, Mississippi (15).
Litton, A., St. Louis, Missouri (12).
Locke, Joseph M., Cincinnati, Ohio (13).
Locke, Luther F., Nashua, New Hampshire (7).
Logan, William E., Montreal, Canada (1).
Loomis, Elias, New Haven, Connecticut (1).
Loomis, L. C., Wheeling, West Virginia (9).

Loomis, Silas L., Washington, District of Columbia (7).
Loosey, Charles F., New York, New York (12).
Lord, John, Stamford, Connecticut (13).
Lothrop, Joshua R., Buffalo, New York (15).
Lovering, Joseph, Cambridge, Massachusetts (2).
Lunn, William, Montreal, Canada (11).
Lyman, B. S., Philadelphia, Pennsylvania (15).
Lyman, Chester S., New Haven, Connecticut (4).
Lynch, P. N., Charleston, South Carolina (2).

M.

Mahan, D. H., West Point, New York (9).
Marcy, Oliver, Evanston, Illinois (10).
Marissal, F. V., Fall River, Massachusetts (14).
* Marsh, Dexter, Greenfield, Massachusetts (1).
Marsh, O. C., New Haven, Connecticut (15).
Marshall, Charles D., Buffalo, New York (15).
Marshall, Orasmus H., Buffalo, New York (15).
* Mather, William W., Columbus, Ohio (1).
Mauran, J., Providence, Rhode Island (2).
Mayhew, D. P., Ypsilanti, Michigan (13).
Maynard, Alleyne, Cleveland, Ohio (7).
M'Conihe, Isaac, Troy, New York (4).
McRae, John, Camden, South Carolina (3).
Meade, George G., Philadelphia, Pennsylvania (15).
Means, A., Oxford, Georgia (5).
Meek, F. B., Washington, District of Columbia (6).
Meigs, James A., Philadelphia, Pennsylvania (12).
Miles, Henry H., Quebec, Canada East (11).
Miller, Samuel, New Haven, Connecticut (14).
Minifie, William, Baltimore, Maryland (12).
Mitchell, Maria, Poughkeepsie, New York (4).
Morgan, DeWitt C., Baltimore, Maryland (14).
Morgan, Lewis H., Rochester, New York (10).
Morris, D., Ellingham, Connecticut (14).
Morris, John G., Baltimore, Maryland (12).
Morris, J. R., Houston, Texas (11).
* Morton, S. G., Philadelphia, Pennsylvania (1).
Munroe, Nathan, Bradford, Massachusetts (6).
Murray, David, New Brunswick, New Jersey (11).

N.

Nason, Henry B., Beloit, Wisconsin (13).
Nelson, Cleland K., Annapolis, Maryland (12).
Newberry, J. S., New York, New York (5).
Newcomb, Simon, Washington, District of Columbia (13).

*Newton, E. H., Cambridge, New York (1).
Newton, Hubert A., New Haven, Connecticut (6).
Newton, Robert S., New York, New York (15).
*Nicollett, J. N., Washington, District of Columbia (1).
Niles, W. H., Cambridge, Massachusetts (16).
*Norton, J. P., New Haven, Connecticut (1).
Norton, W. A., New Haven, Connecticut (6).
Nye, A. H., Buffalo, New York (15).

O.

*Oakes, William, Ipswich, Massachusetts (1).
Oliver, James Edward, New York, New York (7).
*Olmsted, Alexander F., New Haven, Connecticut (4).
*Olmsted, Denison, New Haven, Connecticut (1).
*Olmsted, Denison, Jr., New Haven, Connecticut (1).
Ordway, John M., Providence, Rhode Island (9).

P.

Packard, A. S., Jr., Salem, Massachusetts (16).
Paine, Cyrus F., Rochester, New York (12).
Painter, Minshall, Lima, Pennsylvania (7).
Palmer, Everard, Buffalo, New York (15).
Parker, Henry E., Hanover, New Hampshire (11).
*Parkman, Samuel, Boston, Massachusetts (1).
Parmelee, Dubois D., New York, New York (15).
Parry, Charles C., Washington, District of Columbia (6).
Parvin, Theodore S., Iowa City, Iowa (7).
Pease, Francis S., Buffalo, New York (15).
Peck, Edward W., Burlington, Vermont (16).
Peck, William G., New York, New York (13).
Peirce, Benjamin, Cambridge, Massachusetts (1).
Peirce, James M., Cambridge, Massachusetts (11).
Perkins, George R., Utica, New York (1).
Perkins, Maurice, Schenectady, New York (15).
Perry, John B., Brookline, Massachusetts (16).
*Perry, M. C., New York, New York (10).
Phelps, Almira L., Baltimore, Maryland (13).
Phelps, Charles E., Baltimore, Maryland (13).
Pierce, Henry M., New York, New York (15).
Pierrepont, H. E., Brooklyn, New York (14).
Plant, I. C., Macon, Georgia (3).
*Plumb, Ovid, Salisbury, Connecticut (9).
Poole, Henry W., Boston, Massachusetts (14).
Pope, Charles A., St. Louis, Missouri (12).
*Porter, John A., New Haven, Connecticut (14).
Powell, Samuel, Philadelphia, Pennsylvania (13).

Prescott, William, Concord, New Hampshire (1).
Pruyn, J. V. L., Albany, New York (1).
Pugh, Evan, Centre Co., Pennsylvania (14).
Putnam, F. W., Salem, Massachusetts (10).

Q.

Quincy, Edmund, Jr., Boston, Massachusetts (11).

R.

Rankin, Alexander T., Buffalo, New York (15).
Ranney, Orville W., Buffalo, New York (15).
Rauch, J. H., Chicago, Illinois (11).
Raymond, R. W., New York, New York (15).
Redfield, John H., Philadelphia, Pennsylvania (1).
* Redfield, William C., New York, New York (1).
Rice, Clinton, New York, New York (7).
Richardson, Horace, Boston, Massachusetts (12).
Ritchie, E. S., Boston, Massachusetts (10).
Robertson, Thomas D., Rockford, Illinois (10).
Robinson, Coleman T., Buffalo, New York (15).
Rochester, Thomas F., Buffalo, New York (15).
Rockwell, Alfred P., New Haven, Connecticut (10).
Rockwell, John, La Salle, Illinois (11).
* Rockwell, John A., Norwich, Connecticut (10).
Rodman, William M., Providence, Rhode Island (9).
Rogers, Fairman, Philadelphia, Pennylvania (11).
* Rogers, James B., Philadelphia, Pennsylvania (1).
Rogers, W. A., Alfred Centre, New York (15).
Rogers, W. B., Boston, Massachusetts (1).
Rood, O. N., New York, New York (14).
Roosevelt, Clinton, New York, New York (11).
Rumsey, Bronson C., Buffalo, New York (15).
Rumsey, Dexter P., Buffalo, New York (15).
Russell, Andrew, Ottawa, Canada West (11).
Rutherford, Louis M., 175 2d Av., New York, New York (13).

S.

Safford, J. M., Nashville, Tennessee (6).
Safford, Truman H., Chicago, Illinois (13).
Sanborn, Francis G., Boston, Massachusetts (13).
Sargent, Rufus, Auburn, New York (10).
Scarborough, George, Sumner, Kansas (2).
Schanck, J. Stillwell, Princeton, New Jersey (4).
Schofield, J. M., Washington, District of Columbia (13).
Schott, Arthur C. V., Georgetown, District of Columbia (8).
Schott, Charles A., Washington, District of Columbia (8).

Scudder, Samuel H., Cambridge, Massachusetts (13).
Sellstedt, Laurentius G., Buffalo, New York (15).
Seward, William H., Auburn, New York (1).
Sexton, Jason, Buffalo, New York (15).
Seymour, M. H., Montreal, Canada (11).
Sheafer, P. W., Pottsville, Pennsylvania (4).
Shepard, C. U., Amherst, Massachusetts (4).
Sherwood, Albert, Buffalo, New York (15).
Sias, Solomon, Charlotteville, New York (10).
Sill, Elisha N., Cuyahoga Falls, Ohio (6).
* Silliman, Benjamin, New Haven, Connecticut (1).
Silliman, Benjamin, New Haven, Connecticut (1).
Sillsbee, E. A., Salem, Massachusetts (14).
Skinner, John B., Buffalo, New York (15).
Smith, A. D., Providence, Rhode Island (14).
Smith, J. L., St. Louis, Missouri (14).
* Smith, J. V., Cincinnati, Ohio (5).
Smith, James Y., Providence, Rhode Island (9).
* Smith, Lyndon A., Newark, New Jersey (9).
Snell, Eben S., Amherst, Massachusetts (2).
* Sparks, Jared, Cambridge, Massachusetts (2).
Spencer, Charles A., Brooklyn, New York (14).
Spring, Charles H., Boston, Massachusetts (13).
Squier, George L., Buffalo, New York (15).
Stanard, Benjamin A., Cleveland, Ohio (6).
Stearns, Josiah A., Boston, Massachusetts (10).
Stearns, William F., Oxford, Mississippi (13).
Steele, Oliver G., Buffalo, New York (15).
Steiner, Lewis H., Frederick City, Maryland (7).
Stevenson, Charles L., Charlestown, Massachusetts (14).
Stewart, William W., Buffalo, New York (15).
Stimpson, William, Chicago, Illinois (12).
Stoddard, O. N., Oxford, Ohio (7).
Storer, D. H., Boston, Massachusetts (1).
Storer, Frank H., Boston, Massachusetts (13).
Sullivant, W., S, Columbus, Ohio (7).
Swallow, G. C., Columbia, Missouri (10).
Swinburne, John, Albany, New York (6).

T.

* Tallmadge, James, New York, New York (1).
* Taylor, Richard C., Philadelphia, Pennsylvania (1).
* Teschemacher. J. E., Boston, Massachusetts (1).
Thomas, Calvin F. S., Buffalo, New York (15).
Thompson, Alexander, Aurora, New York (6).
Thompson, Aaron R., New York, New York (1).

* Thompson, Z., Burlington, Vermont (1).
* Thurber, Isaac, Providence, Rhode Island (9).
Tillman, S. D., New York, New York (15).
Tingley, Joseph, Meadville, Pennsylvania (15).
Tingley, Joseph, Greencastle, Indiana (14).
Tolles, Robert B., Boston, Massachusetts (15).
Torrey, John, New York, New York (1).
* Totten, J. G., Washington, District of Columbia (1).
Townsend, Franklin, Albany, New York (4).
* Townsend, John K., Philadelphia, Pennsylvania (1).
* Troost, Gerard, Nashville, Tennessee (1).
Trowbridge, W. P., New York, New York (10).
* Tuomey, M., Tuscaloosa, Alabama (1).
Turner, Henry E., Newport, Rhode Island (14).
* Tyler, Edward R., New Haven, Connecticut (1).
Tyler, P. B., Springfield, Massachusetts (13).
Tyson, Philip T., Baltimore, Maryland (12).

U.

Upham, J. Baxter, Boston, Massachusetts (14).

V.

* Vancleve, John W., Dayton, Ohio (1).
Van Duzee, Mary K., Buffalo, New York (16).
Van Pelt, William, Williamsville, New York (7).
Vanuxem, Lardner, Bristol, Pennsylvania (1).
Vaux, William S., Philadelphia, Pennsylvania (1).
Verrill, A. E., New Haven, Connecticut (16).
Viele, Henry K., Buffalo, New York (15).
Vose, George L., Paris Hill, Maine (15).
Vought, John H., Buffalo, New York (15).

W.

* Wadsworth, James S., Genessee, New York (2).
* Wagner, Tobias, Philadelphia, Pennsylvania (9).
Wales, Torrey E., Burlington, Vermont (16).
Wales, William, Fort Lee, New York (15).
Walker, Joseph, Oxford, New York (10).
* Walker, Sears C., Washington, District of Columbia (1).
* Walker, Timothy, Cincinnati, Ohio (4).
Walling, H. F., Easton, Pennsylvania (16).
Ward, Henry A., Rochester, New York (13).
Warner, H. G., Rochester, New York (11).
Warren, G. K., Washington, District of Columbia (12).
* Warren, John C., Boston, Massachusetts (1).
Watson, James C., Ann Arbor, Michigan (13).

Watson, William, Cambridge, Massachusetts (12).
* Webster, H. B., Albany, New York (1).
* Webster, J. W., Cambridge, Massachusetts (1).
* Webster, M. H., Albany, New York (1).
Webster, Nathan B., Kenansville, North Carolina (7).
Wenz, J., New Orleans, Louisiana (15).
West, Charles E., Brooklyn, New York (1).
Wheatland, Henry, Salem, Massachusetts (1).
* Wheatland, Richard H., Salem, Massachusetts (13).
Wheatley, Charles M., Phœnixville, Pennsylvania (1).
Wheeler, T. B., Montreal, Canada (11).
Wheildon, W. W., Charlestown, Massachusetts (13).
White, Henry H., Harrodsburg, Kentucky (14).
Whitney, Asa, Philadelphia, Pennsylvania (1).
Whitney, H. H., Montreal, Canada (11).
Whitney, Josiah D., Cambridge, Massachusetts (1).
Whitney, William D., New Haven, Connecticut (12).
Whittlesey, Charles, Cleveland, Ohio (1).
Willard, Emma, Troy, New York (15).
Williams, Henry W., Boston, Massachusetts (11).
Williams, Matthew, Syracuse, New York (13).
Williamson, R. S., San Francisco, California (12).
Willmarth, A. F., New York, New York (15).
Wilson, Daniel, Toronto, Canada (10).
Winchell, Alexander, Ann Arbor, Michigan (3).
* Woodbury, L., Portsmouth, New Hampshire (1).
Worthen, A. H., Springfield, Illinois (5).
Wright, A. W., New Haven, Connecticut (14).
Wright, Chauncey, Cambridge, Massachusetts (9).
* Wright, John, Troy, New York (1).
Wurtele, Louis C., Acton Vale, Canada East (11).
Wurtz, Henry, New York, New York (10).
Wyckoff, C. C., Buffalo, New York (15).

Y.

Youmans, E. L., New York, New York (6).
* Young, Ira, Hanover, New Hampshire (7).

This list contains five hundred and thirty-one names, of which one hundred and three are of deceased members. The names of those who were chosen at Chicago, and who have already joined the Association, have not yet been incorporated into the general catalogue of members, but are printed separately on the following pages.

MEMBERS

WHO JOINED AT

THE CHICAGO MEETING.

A.

Adams, O. N., La Salle, Illinois.
Allen, Harrison, Philadelphia, Pennsylvania.
Allen, Jerome, Monticello, Iowa.
Allen, William T., Chicago, Illinois.
Alvord, Benjamin, Omaha, Nebraska.
Andrews, Ebenezer, Chicago, Illinois.
Andrews, Edmund, Chicago, Illinois.
Andrews, Joseph H., Chicago, Illinois.
Appleton, Ellery C., Boston, Massachusetts.
Armour, George, Chicago, Illinois.
Atwater, Mrs. Elizabeth E., Chicago, Illinois.
Atwater, Samuel T., Chicago, Illinois.
Atwater, W. C., New Haven, Connecticut.

B.

Babcock, Henry H., Chicago, Illinois.
Baird, Lyman, Chicago, Illinois.
Ballard, Edward, Chicago, Illinois.
Bannister, Henry M., Evanston, Illinois.
Bartlett, John, Keokuk, Iowa.
Beal, William J., Chicago, Illinois.
Beaty, Jonathan F., Chicago, Illinois.
Beebe, Edward H., Galena, Illinois.
Beebe, G. D., Chicago, Illinois.
Beecher, Jerome, Chicago, Illinois.
Bickmore, Albert S., St. George, Maine.
Bill, Charles, Chicago, Illinois.
Blatchford, Eliphalet W., Chicago, Illinois.
Bolles, E. C., Brooklyn, New York.
Bolter, Andrew, Chicago, Illinois.
Bolton, H. C., New York, New York.
Bowen, Chauncey T., Chicago, Illinois.

Bowen, James H., Chicago, Illinois.
Bradley, Francis, Chicago, Illinois.
Bradley, Frank H., New Haven, Connecticut.
Briggs, S. A., Chicago, Illinois.
Brigham, Charles H., Ann Arbor, Michigan.
Bristol, Eugene S., New Haven, Connecticut.
Brown, Mrs. Carrie R., Cincinnati, Ohio.
Brownne, Mrs. A. B., New York, New York.
Brownne, Robert H., New York, New York.
Bryan, Thomas B., Chicago, Illinois.
Bullock, W. H., Chicago, Illinois.
Burley, A. H., Chicago, Illinois.
Buttrick, E. L., Chicago, Illinois.

C.

Calhoun, John B., Chicago, Illinois.
Campbell, R. A., Chicago, Illinois.
Canby, William M., Wilmington, Delaware.
Carter, Asher, Chicago, Illinois.
Case, L. B., Richmond, Indiana.
Chanute, O., Kansas City, Kansas.
Chapman, F. M., Chicago, Illinois.
Clark, John E., Yellow Springs, Ohio.
Clarke, William H., Chicago, Illinois.
Coelln, C. W. von, Grinnell, Iowa.
Colbert, E., Chicago, Illinois.
Collett, John, Eugene, Indiana.
Cook, A. J., Lansing, Michigan.
Cooke, Josiah P., Cambridge, Massachusetts.
Cope, Edward D., Philadelphia, Pennsylvania.
Corse, John M., Chicago, Illinois.
Cowles, Alfred, Chicago, Illinois.
Cram, Charles H., Chicago, Illinois.
Crerar, John, Chicago, Illinois.
Crosby, Uranus H., Chicago, Illinois.
Culver, Bilden F., Chicago, Illinois.
Culver, Howard L., Chicago, Illinois.
Cutting, Hiram A., Lunenburg, Vermont.

D.

Daniels, Edward, Iona, Virginia.
Daniels, W. W., Madison, Wisconsin.
Davies, John E., Madison, Wisconsin.
Davis, N. S., Chicago, Illinois.
Dillingham, W. A. P., Augusta, Maine.
Dodson, N. M., Berlin, Wisconsin.

Doggett, Mrs. Kate N., Chicago, Illinois.
Doggett, William E., Chicago, Illinois.
D'Ooge, Martin L., Ann Arbor, Michigan.
Douglas, J. M., Chicago, Illinois.
Duncan, T. C., Chicago, Illinois.
Durham, Benjamin, Chicago, Illinois.

E.

Eaton, James H., Amherst, Massachusetts.
Eberhart, John F., Chicago, Illinois.
Edwards, J. B., Montreal, Canada.
Egleston, Thomas, Jr., New York, New York.
Ely, John F., Cedar Rapids, Iowa.
Embeck, William, St. Louis, Missouri.
Emery, Rush, Iowa City, Iowa.
English, William J., Ann Arbor, Michigan.
Everett, Oliver, Dixon, Illinois.
Everts, W. W., Chicago, Illinois.

F.

Farnsworth, P. J., Clinton, Iowa.
Farrar, Henry W., Cambridge, Massachusetts.
Ferry, William H., Jr., Chicago, Illinois.
Fisher, Davenport, Milwaukee, Wisconsin.
Foster, Henry, Clifton, New York.
Foster, John, Schenectady, New York.
Freeman, H. C., South Pass, Illinois.

G.

Gamgee, John, London, England.
Gaylord, Frederick, Chicago, Illinois.
Gill, Theodore, Washington, District of Columbia.
Goodwin, Daniel, Chicago, Illinois.
Gove, Edward, Portland, Maine.
Greeley, Samuel S., Chicago, Illinois.
Green, Oliver B., Chicago, Illinois.
Greene, Dascom, Troy, New York.
Greenebaum, Henry, Chicago, Illinois.
Grimes, J. Stanley, New York, New York.
Grover, Z., Chicago, Illinois.

H.

Hagen, Hermann A., Cambridge, Massachusetts
Hale, Edwin M., Chicago, Illinois.
Hall, P. A., Chicago, Illinois.
Halsey, Le Roy J., Chicago, Illinois.

Hamilton, Hugh, Harrisburg, Pennsylvania.
Hammer, A., St. Louis, Missouri.
Hammond, C. G., Chicago, Illinois.
Harris, Mrs. A. A., Cambridge, Massachusetts.
Harris, Clarendon, Laporte, Indiana.
Harris, Edward D., Cambridge, Massachusetts.
Harris, James W., Cambridge, Massachusetts.
Haven, Joseph, Chicago, Illinois.
Hawkins, B. W., New York, New York.
Hay, Walter, Chicago, Illinois.
Hayes, John L., Cambridge, Massachusetts.
Hendrick, Joel, Hornellsville, New York.
Hibbard, John R., Chicago, Illinois.
Hickcox, S. V. R., Chicago, Illinois.
Hildreth, Joseph S., Chicago, Illinois.
Hilgard, Theodore C., St. Louis, Missouri.
Hill, E. A., Chicago, Illinois.
Hilton, John C., Chicago, Illinois.
Hinrichs, Gustavus, Iowa City, Iowa.
Hitt, Isaac R., Chicago, Illinois.
Hobart, E. F., Beloit, Wisconsin.
Holmes, E. L., Chicago, Illinois.
Holstein, Charles H., Indianapolis, Indiana.
Horsford, M. L., Cambridge, Massachusetts.
Hoy, Philo R., Racine, Wisconsin.
Hubbard, Charles E., Boston, Massachusetts.
Hubbard, Gurdon S., Chicago, Illinois.
Hubbard, Sara A., Kalamazoo, Michigan.
Hunt, Charles S., New York, New York.
Hunt, W. C., Chicago, Illinois.
Huse, Frederick J., Evanston, Illinois.

J.

Johnson, H. A., Chicago, Illinois.
Judd, N. B., Chicago, Illinois.

K.

Katte, Walter, Chicago, Illinois.
Kedzie, J. H., Chicago, Illinois.
Keissell, A. S., Davenport, Iowa.
Klippart, John H., Columbus, Ohio.
Knickerbocker, Charles, Chicago, Illinois.
Koschkull, F. von, Tiflis, Russia.
Kost, J., Henry, Illinois.

L.

Lane, E. S., Sandusky, Ohio.

Lathrop, E. L., Chicago, Illinois.
Leakin, George A., Baltimore, Maryland.
Lincoln, Robert T., Chicago, Illinois.
Lombard, Benjamin, Chicago, Illinois.
Loring, Sanford E., Chicago, Illinois.
Lovering, James W., Cambridge, Massachusetts.
Ludlam, R., Chicago, Illinois.
Lunt, Orrington, Chicago, Illinois.
Lupton, N. T., Greensboro, Alabama.
Lyman, Henry M., Chicago, Illinois.

M.

McCagg, Ezra B., Chicago, Illinois.
McDermott, Michel, Chicago, Illinois.
McLain, B. W., Fort Wayne, Indiana.
McMurtrie, Horace, Boston, Massachusetts.
Magoun, George F., Grinnell, Iowa.
Mannheimer, M., Chicago, Illinois.
Marsh, H. H., Chicago, Illinois.
*Marsh, James P., Chicago, Illinois.
Meehan, Thomas, Germantown, Pennsylvania.
Meeker, Arthur B., Chicago, Illinois.
Meysenburg, T. A., St. Louis, Missouri.
Miller, Joseph A., New York, New York.
Mitchell, William H., Florence, Alabama.
Moran, Charles, New York, New York.
Myers, W. H., Fort Wayne, Indiana.

N.

Nichols, Charles A., Providence, Rhode Island.
Norwood, J. G., Columbia, Missouri.

O.

Officer, Alexander, Chicago, Illinois.
Ogden, Mahlon D., Chicago, Illinois.
Ogden, William B., Chicago, Illinois.
Olmstead, Lemuel G., Moreau Station, New York.

P.

Page, Peter, Chicago, Illinois.
Park, Roswell, Chicago, Illinois.
Parkes, Charles T., Chicago, Illinois.
Peabody, S. H., Chicago, Illinois.
Peck, W. F., Davenport, Iowa.
Pennington, L. S., Sterling, Illinois.
Perkins, George H., New Haven, Connecticut.

Phillips, E. B., Chicago, Illinois.
Powell, Edwin, Chicago, Illinois.
Pratt, William H., Davenport, Iowa.
Prentiss, A. N., Lansing, Michigan.
Pumpelly, Raphael, Owego, New York.

Q.

Quinn, David, Chicago, Illinois.

R.

Read, Daniel, Alton, Illinois.
Read, Daniel, Columbia, Missouri.
Reed, Alexander, Philadelphia, Pennsylvania.
Reed, Caroline G., New York, New York.
Riley, Charles V., St. Louis, Missouri.
Robinson, Lucy, London, Canada.
Rockwell, Joseph P., New Haven, Connecticut.
Root, Edward W., Clinton, New York.
Rumsey, George F., Chicago, Illinois.
Rumsey, Julian S., Chicago, Illinois.
Russell, Jacob W., Chicago, Illinois.
Ryerson, Joseph T., Chicago, Illinois.

S.

Saunders, William, London, Canada.
Saunders, Mrs. William, London, Canada.
Sawyer, Charles B., Chicago, Illinois.
Scammon, J. Young, Chicago, Illinois.
Sheffield, Charles J., New Haven, Connecticut.
Sheldon, Edwin H., Chicago, Illinois.
Shepard, Charles U., Jr., Amherst, Massachusetts.
Skinner, E. S., Chicago, Illinois.
Smith, D. S., Chicago, Illinois.
Smith, James A., Chicago, Illinois.
Spalding, Jesse, Chicago, Illinois.
Sprague, Albert A., Chicago, Illinois.
Stallkneckt, F., New York, New York.
Stewart, Hart L., Chicago, Illinois.
Stockwell, John N., Brecksville, Ohio.
Stone, Samuel, Chicago, Illinois.
Storrs, Henry M., Brooklyn, New York.
Swasey, Oscar F., Beverly, Massachusetts.

T.

Tenney, Sanborn, Williamstown, Massachusetts.
Thompson, Harvey M., Chicago, Illinois.

Thompson, S. H., Hanover, Indiana.
Townshend, Norton S., Avon, Ohio.
Tracy, John F., Chicago, Illinois.
Trembley, J. B., Toledo, Ohio.
True, Nathaniel T., Bethel, Maine.
Tucker, William F., Chicago, Illinois.
Turner, John B., Chicago, Illinois.
Tuttle, Albert H., Cleveland, Ohio.

U.

Upton, George P., Chicago, Illinois.

W.

Wagner, William, Chicago, Illinois.
Walker, Charles H., Chicago, Illinois.
Walker, George C., Chicago, Illinois.
Walsh, Benjamin D., Rock Island, Illinois.
Ward, R. H., Troy, New York.
Warder, John A., Cincinnati, Ohio.
Warren, S. Edward, Troy, New York.
Welch, A. S., Iowa Agricultural College, Ames, Iowa.
Welch, Rodney, Chicago, Illinois.
Weyde, P. H. Van der, New York, New York.
White, Charles A., Iowa City, Iowa.
Whitford, W. C., Milton, Wisconsin.
Williams, Norman, Jr., Chicago, Illinois.
Wilson, Charles L., Chicago, Illinois.
Winslow, Ferdinand S., Chicago, Illinois.
Winslow, J. C., Danville, Illinois.
Wood, Horatio C., Jr., Philadelphia, Pennsylvania.
Wood, John H., Chicago, Illinois.
Woods, John, Philadelphia, Pennsylvania.
Woodworth, John M., Chicago, Illinois.

Y.

Young, John Jay, Chicago, Illinois.

Z.

Zaremba, Charles W., Chicago, Illinois.

NOTE.—Whole number of new members two hundred and fifty-eight.

235 V.

BOSTON MEDICAL
JUL 18 1892
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ADDRESS

IN COMMEMORATION OF

ALEXANDER DALLAS BACHE,

BY

BENJAMIN APTHORP GOULD.

GENTLEMEN OF THE AMERICAN ASSOCIATION FOR THE ADVANCEMENT OF SCIENCE:—

IT is no common event which you have called upon me to commemorate. Death, who comes with an impartial tread into the hovels of the poor and the castles of monarchs, is a visitor too well known to us all. Yet, sometimes he assumes a new aspect. When, three years ago, at the very moment of the hard won and stern rejoicing of the Republic, we saw her first citizen struck down in the midst of the people who honored him, the destroyer gained almost a new power in our imaginations. So is it with us now. He, who by common consent, unquestioning and unchallenged, stood forth preëminent as our leader in science, our first counsellor where her welfare was at stake, unflinching in the maintenance of her interests, wise in the guidance of her affairs,—he has gone from among us.

Adequately to portray the character, abilities and influence of such a man would be an undertaking from which the ablest tongue or pen might shrink. It is not for the labor it implies, for labor is fitly bestowed in recording a life at once so great and so inspiring; not from the natural timidity, which even the most competent might well feel, in presuming to pronounce any judgement upon so rare a virtue, ability and patriotism; nor is it even that our point of view is palpably too near, to permit the just portrayal of this lofty character with all the truthfulness of outline for which a comprehensive survey of the whole in its many relations is imperative; or to give its fair proportions, undistorted by the mists which encompass our vision, and untinged by the hues with which affection adorns his image. Beside all these, there is yet another reason for distrust. His influence extended through so large a sphere that it is difficult for us fully to comprehend it now. The more we examine the tokens of its action, the more do we become impressed with its extraordinary range. I know of no department of physical or natural science which has not been stimulated or fostered through his means. The legislative and executive departments of the nation knew his power through many years, and relied upon it in matters far beyond the range of his ordinary pursuits. Both the army and navy felt, and have often acknowledged their obligations to him. The progress of education, the development of scientific research, the extent of scientific discovery, the growth of the arts, and the spread of commerce, have all been greater in America because he has lived.

Such a man was our beloved and honored BACHE. To hesitate, when summoned to put into words our common tribute, were unworthy of his friend or of his pupil. You will all feel the inadequacy of the offering, and the futility of attempting to compress into the utterance of an hour or two such records and such results. Many of you have already brought him a better tribute,—that of years of fruitful labor, prompted and encouraged by himself, or of a change in the aims and pursuits of a lifetime, induced by his wise and kindly counsel.

ALEXANDER DALLAS BACHE was born in Philadelphia on the 19th day of July, 1806. If intellectual and moral eminence might be inferred from an honorable lineage, which, unhappily, is not always the case, it certainly would have been anticipated for him. His father was the son of Richard Bache and Sarah Franklin; the former, President of the Republican Society of Philadelphia at the outbreak of the American Revolution, and Postmaster General of the United States from 1776 to 1782; the latter, the only child of Benjamin Franklin and his wife Deborah (Read), and herself eminent as one of the heroines of the war of independence. The noble women who, during our recent struggle for the maintenance of the nationality then achieved, gave their time and energies to the support of their countrymen facing the edge of battle, and who have aided in making the name of our own Sanitary Commission immortal, were but the unconscious imitators of that smaller but equally devoted band who, during our first struggle for national existence, similarly labored in mitigating the sufferings of our soldiers. Like them, Mrs. Bache ministered to the sick and wounded in army hospitals; and under her superintendence more than 2200 women were at one time employed in making garments for the barefooted and half-clad men, who, against almost unparalleled obstacles, were achieving the independence of a continent and a new vantage-ground for the oppressed of all nations. The maternal grandfather of our departed colleague, for whom he received his name, was Alexander James Dallas, Secretary of the Treasury of the United States from Oct. 1814 to Nov. 1816, who, in the second war of our Republic, redeemed its finances from confusion, and, in the short space of two years, restored them to a condition of order and stability. Under his administration also, the Coast Survey of the United States was established, and Mr. Hassler appointed to its superintendence. The late Commodore Dallas, and Mr. George M. Dallas, Vice President of the United States, were his sons.

A peculiarly large number of Mr. Bache's family, both on the paternal and maternal side, were engaged in the government service, civil or military, and young Bache was destined by his father for the army, which offered also to the boy an attractive career. Accordingly, at the age of fifteen, he entered the Military Academy at West Point, where he graduated, July 1, 1825, the first scholar in a class so far above the usual grade of excellence that four of its members were assigned to the Corps of Engineers, although for more than one or two in each class to attain this distinction was a rare occurrence. Among his classmates at West Point were General Robert Anderson, of Fort Sumter, and Major General C. F. Smith, who served his native land so faithfully and effectively during the recent rebellion.

It is recorded of Bache that, during the whole term of his course at the Military Academy, he never incurred a single mark of demerit. And this is all the more to his credit, in that he was no demure and prematurely sedate youth, but possessed, in an eminent degree, that love of frolic and of jest which formed a prominent trait of his character in riper years. Nor was the high position which he acquired in the confidence and respect of his instructors attained at the expense, in any degree, of the affection of his fellow pupils. The bonds of friendship formed within the precincts of the Military Academy seem to have been of remarkable strength, and were most tenderly guarded by him throughout his subsequent life.

The final examination of the cadets, just before the close of their academic course, was attended then, as now, by prominent officers of the military service. In the year now referred to, the Secretary of War himself was present, and the tokens of scholarly and military excellence given by the first pupil of the class were such as to elicit the most uncommon marks of approval. The Secretary himself, with a sympathetic appreciation which does him the highest honor, was prompted to the unusual step of addressing a letter to Bache's mother. This letter, one of that mother's most cherished mementoes, I am permitted to read to you.

WEST POINT, June 10th, 1825.

My Dear Madam:—Upon any other occasion than the subject of the present letter, I should be obliged to admit that our very small acquaintance would rebuke me for addressing you. But, being a father, and knowing how exquisite is the pleasure arising from the well-doing of children, I am quite sure, from your amiable disposition, that you will forgive me, when you learn that my only purpose in writing is to state, as I do most sincerely, how greatly I was gratified at the evidences given by your son in his examination, of the excellence of his attainments. He ought to be, as I am sure he will be, a source of the greatest consolation. I know not whether it has been your lot to have your cup of life drugged in any degree with calamity. The draught must have been severe indeed if it is not sweetened by the blessing of your excellent son. I knew and loved your father—his great paternal ancestor I knew only by his works. I thought, or permitted myself to believe, that I saw the excellences of both branches about to be united in your son. I offer you my sincere congratulations.

JAMES BARBOUR.

MRS. SOPHIA BACHE.

That mother's cup of life was indeed bitterly drugged with calamity; but the supporting arm of her son, the rich honors which she happily lived to see accorded him in no stinted measure, and the abundant benefits to the commonwealth and to the nation, which followed his accession to each successive place of influence or authority, did most effectively sweeten it, and cause it to run over with gladness.

During the first year after graduating, he remained at West Point, as assistant to Professor Mahan in the department of Engineering, and, in the summer of 1826, was detailed as assistant to Colonel Totten, then in charge of the construction of Fort Adams, at Newport. Here commenced a close friendship between these two eminent men, which increased with their increasing years, and was severed only by death. And when, long years afterwards, their duties brought them once again to the same place of residence, and permitted a resumption of their intimate communion, the joint influence of General Totten, as Chief of the Engineer Corps, and of Professor Bache, as Superintendent of the Coast Survey, quietly but steadily wrought a wondrous change in the welfare of scientific interests, and in the position of scientific men at Washington. In their intimate domestic intercourse they not only occupied the relation, but assumed the titles of kindred; and to their mutual support in times of peril to the great intellectual interest which they defended, American science will be forever indebted.

Nor was General Totten the most important or the nearest friend whom Bache won for life during his sojourn of two years at Newport. It was his privilege there also to enlist the affection and secure the hand of an admirable woman, who, for nearly forty years, accompanied him in all his many wanderings; by her sound judgement, unsurpassed devotion, and intellectual ability, multiplying his opportunities of usefulness as well as his happiness, and rendering it possible for him to accomplish, for his own honor and for the welfare of his country, what no man probably could have accomplished without some such assistance.

On the 16th of September, 1828, only three years after graduating from West Point, and only two years after the commencement of his professional duties at Fort Adams, Mr. Bache was elected Professor of Natural Philosophy and Chemistry in the University of Pennsylvania, at Philadelphia. He was at this time but twenty-two years old, yet the repute of his career at West Point had been already a source of pride to his native city, and sundry communications, which he had found time in Newport to contribute to the *Mathematical Journal*, had borne witness to his mental activity and ability. Mr. Bache obtained a six months' leave of absence, was married as soon as it was received, and, on the 11th of October, about three weeks after his election, arrived in Philadelphia to assume the duties of his professorship. These duties proving acceptable to himself and to the college authorities, he definitely resigned his position in the army at the expiration of his leave of absence.

Here commences his scientific career, interrupted sometimes by the pressure of other duties, but never discontinued, no matter what his avocations, so long as God granted him and us a continuance of his mental powers.

For seven years Professor Bache retained his position in the University, beloved by his pupils, esteemed by his colleagues, respected by the whole community. During these years he was an active member of the Franklin Institute and of the Philosophical Society, and their transactions contain some twenty-five contributions from him within this period, all of them recording the results of original scientific research.

Among these papers some deserve especial mention.

The earliest which I find recorded is an article "On the Specific Heat of the Atoms of Bodies," published in February, 1829. In this firstling he maintained that the best and latest determinations of the atomic weights and specific heats of elements failed to support the doctrine that the specific heat of the atoms is the same for all bodies. The topic was a large one for discussion by a young man of twenty-two, but he certainly made his point good; and if the theory which he then opposed has found acceptance in subsequent years, it has only been because the data upon which it rests have been modified by more accurate determination.

This paper was followed, a year later, by his first experimental research, which was upon the inflammation of phosphorus in a vacuum, or rather, in a highly rarified medium. This appeared in the *American Journal of Science*, bearing date May, 1830, and was the beginning of a more extended line of investigation, subsequently followed up. A stage was reached, at last, where it became requisite to learn the order of thermal conducting power for various powders, since the research depended on the degree to which that small amount of heat could be retained, which was generated by means of what little oxygen remained for contact with the phosphorus. In the air many powders were capable of thus producing inflammation; and exhaustion of the air operated simply by removing a conveyer of heat. These conducting powers were not determined, and the research remained unpublished.

The influence of the Franklin Institute, in giving to Bache's first researches an especially practical character, is very noticeable at this period. General Mechanics received a large share of his attention. An article which he printed in April, 1831, proposes a safety-apparatus for steamboats, composed of a combination of the French plan of plates of fusible metal, with the ordinary safety-valve, so that on the melting of the plate the supply of steam might not be totally lost, thus obviating the main objection to the employment of fusible metal. A year later, he proposed an alarm to be applied to the interior flues of steam boilers. In this, the ordinary difficulties attending the use of fusible metal for such purposes are obviated, by inclosing it in a tube, and imbedding in it the lower end of a rod, to the upper end of which is applied some retractive force. The fusion of the metal releases the rod, causing a bell to ring, or a whistle to sound; after which the rod may be restored to its place, and the metal allowed again to solidify around it. Thus the metal is not exposed to the contact or pressure of the steam, but only to its heating agency. Experiments tried with this apparatus a year or two later, by the Committee of the Franklin Institute on Explosions of Steam Boilers, led to a very favorable report, accompanied by the suggestion that it is equally applicable to boilers without interior flues, and might be employed as a manageable and useful check in ordinary cases upon the safety-valve.

In the beginning of the year 1832, Mr. Saxton, who was then in London, wrote to friends in Philadelphia an early account of Faraday's discovery of magneto-electricity, and of his own repetition of the experiments, as well as those of Nobili made soon afterwards. Apparatus was immediately constructed for repeating the experiment, both in the English and Italian form, and careful observations were published by Professor Bache in the Memoirs of the Franklin Institute for July. His inferences were most philosophically and accurately drawn, and entirely accord with the laws now so well known. What a field has been traversed in the progress of our physical knowledge since that not very distant day! The production of a spark by magnetic agency was a vast step toward making possible the quantitative demonstration of the Correlation of Physical Forces. But Saxton, in writing, carefully says, "a spark resembling the electric spark;" and Bache's first inquiries were directed to solving the great question whether it were indeed electrical in its nature. A quarter-century later, and these sparks, "resembling the electric spark," were flashing messages of peace and good will to men between the home of Faraday and the home of Bache.

I pass over his translations of important recent physical and chemical memoirs, which did good in their day by bringing the newer results of science home to our doors, at a time when familiarity with modern languages was comparatively a rare accomplishment, and so also his experiments on the Navigation of Canals by Steam; but must comment, for an instant, on the interesting and valuable results of his experiments on the correctness of measurement of rainfalls made by the ordinary gauges. These observations were made in 1833, although not published until five years later, when he communicated them to the British Association at its meeting in Newcastle, which he attended. These experiments were very labored, and led to interesting deductions. He placed rain-gauges at each corner of Beck's shot tower, in Philadelphia, at a height of 162 feet from the ground. The locality was favorable, since Philadelphia is situated upon an extensive plain, where neither the falling rain nor the wind encountered purely local influences. The observations were made at first at three different heights, the original motive of the investigation having been, to determine the extent to which the amount of rain collected is affected by the height of the station. It soon became manifest that the effect of eddy-winds was greater than that of the difference of elevation; and Professor Bache himself became convinced that the variation in quantity of rain collected at different heights was chiefly due to this previously unsuspected source of error. The quantities of rain collected at the different angles of the tower varied very greatly; and, in one extreme case, during a westerly wind, the quantity in the gauge at the south-east angle was 2⅓ times as great as that in the north-west gauge. In general, the leeward gauge received more than the windward one. And since the heavier rains generally fall in connection with certain particular directions of the wind, it is clear that averages derived from observations of these rains are liable to constant errors, in the determination of the quantity of rain falling at a given height from the ground. Regarding the supposed variation in quantity as dependent upon variation in height, the results were far less marked; and, although the temperature of the rain-drops was often found higher than that of the air through which they were falling, he states that he never saw a case in which the gauges at the top of the tower showed more rain than those below. The extensive details of this research have not been printed. It was far from Bache's usage to send his papers abroad for publication, but these results were, with entire propriety, communicated to the eminent foreign scientific body whose sessions he was attending, a welcome and honored guest. Speaking of them in later years, he says: "The experiments have never been published *in extenso*, because I thought the cream of them had been taken off, and there was no use for the skim-milk!"

But the subject which, not only at this period but throughout his life, seemed to possess for him the greatest attraction was Terrestrial Magnetism, then made especially prominent as a field for physical investigation, through the experimental and theoretical discoveries of Gauss and Weber. As early as the year 1830, he had equipped a little magnetic observatory in the garden attached to his house, in which regular observations were made during a period of four or five years. In his journeys he carried with him portable instruments with which he determined the magnetic constants of the points visited. What he accomplished in later years for this favorite branch of science, the world knows; and it is certainly not too much to say that, of what we know to-day of the distribution, intensity, and periodic and secular changes of terrestrial magnetism, we are indebted quite as much to Bache as to any other one man.

His first memoir on the subject was presented to the American Philosophical Society in November, 1832, and contains the results of hourly observations of the declinations, in which he was assisted by Professor Frazer. These observations were made with a very long needle, provided with a graduated arc at each end. Beside some small notices of the observed influence of Auroras upon the needle, a phenomenon regarding which facts were then wanting, he published, in the Transactions of the Philosophical Society, in connection with Professor Courtenay (at that time his colleague in the University of Pennsylvania), two very elaborate memoirs on the Dip and Horizontal Intensity of Terrestrial Magnetism at several places in the United States. The care and thoroughness with which these observations were arranged, and the reductions effected, could with difficulty be surpassed.

The illusive promises of brilliant results, so familiar to all students of the physical sciences, and which present such temptations to premature inferences, were not wanting in these magnetic investigations; but Bache, though a young man, was truly a philosopher,—as cautious in his deductions as he was keen in following up the track of an unusual phenomenon or the varied suggestions to which renewed experiment might afford a clue. These earlier observations exhibited an apparent connection between the weather and the amount of variation of the needle. Even the approach of a summer shower seemed to be announced by changes in the magnet. On the morning of the great meteoric display in 1833, Mr. Bache thought he had detected an unusual disturbance,—the needle, which was generally kept in the house, having been carried out to the observatory for greater convenience. But the same transfer, on subsequent mornings, produced the same effect, an effect afterwards referred to changes of temperature; and instruments within doors gave, through several seasons, different results from those observed out of the dwelling-house. Whether the changes of temperature occasioned an alteration in the magnetic axis of the needle, or to what other agency the result was attributable, remained undecided.

During the solar eclipse of 1834, before he had received the Gaussian apparatus, he carried out a careful series of observations to ascertain whether the magnetic forces were disturbed during the eclipse. For these observations he employed the long declination-needle already described, a horizontal bar vibrating in a rarefied medium, and a dipping-needle poised on a knife-edge. Writing of it nine years later, he says: "I had nearly cried 'Eureka' once, but had occasion afterwards to doubt." And again, in a tone almost of sadness, he adds: "The ordinary labors in magnetism are like those in astronomy; they yield no point of discovery, but go to the general accumulation of facts." To what extent he aided in such accumulation in later years, you know.

The observations of magnets during auroral displays promised, at the time, important results; and Faraday encouraged him to proceed in the same course. The formation of an auroral arch appeared to bring the declination-needle to a "point"; but the difficulties arising from the conflict of the testimony of different instruments were found very serious. To obviate these, he resorted to the mode of observation already alluded to, in which the needle was suspended in an atmosphere rarefied to the pressure of from three to three and one-half inches of mercury, a method described in the memoir cited, and which proved of importance for the light bars of Hansteen and others, before Gauss had shown the importance of heavy bars, and thus brought about their general use. Of this same memoir he makes a remark, in the letter to Professor Henry, from which I have already quoted, which may touch a sympathetic chord in the hearts of some of the seniors before me, who recall the intellectual position of the American investigator at epochs by no means so remote as this, of thirty-two years ago.

"This memoir," he says, "has been voted prolix. The reason for my making it in such detail was a sneering remark, that American observations were not given with the necessary detail to enable observers in Europe to judge of their value. Perhaps I erred in being so circumstantial. The neglect with which my friend, Major Sabine, treated these observations, on the plea that I had not combined the dip with them, and that they were over too limited an extent of country, hurt me much; and, in a controversy before the British Association, I charged upon him without drawing bridle, and with a *temporary* effect. The dip observations I was not satisfied with, at the time, having exaggerated ideas of the accuracy attainable in such things. When I saw other instruments and observations, I found that I had been unnecessarily fastidious."

Early in 1833, he published an edition of Sir David Brewster's Treatise on Optics, with Notes and an extended Appendix, giving the mathematical investigation of subjects concerning which the author had only given the results, in popular form.

In May, 1833, a bill having been introduced into the Legislature of Pennsylvania establishing and regulating the standard of Weights and Measures, then not fixed by the Federal government, the House of Representatives referred the bill to the Franklin Institute for their opinion. The Institute referred the matter to a special committee of nineteen, of which Mr. Bache was chairman. Their report was prepared by him, as well as a special analysis of the various reports previously made on Weights and Measures in the United States.

This report is worthy of especial note, since it doubtless contributed to hasten that desirable consummation, the establishment, by Congress, of a uniform standard of Weights and Measures, for which the power had been expressly delegated by the constitution. It took the strongest ground in favor of such Congressional action.

"So impressed are the committee with this view," says the report, "that they would express it as their decided opinion that the most imperfect system of Weights and Measures which has ever been framed, would, if applied in all the States of our Union, be preferable to the most perfect system which should be adopted by any one commonwealth singly. . . . Indeed, in most of the laws of more recent origin adopted by the several States, there is a distinct provision that when Congress shall furnish a system of Weights and Measures for the United States, the temporary State standards shall be made to conform to the national standard. The exceeding importance of uniformity is thus distinctly set forth from quarters of the highest authority in the different parts of our extended republic."

The report, which was signed by all the members of the committee, and unanimously adopted by the Board of Managers of the Institute, concluded by recommending that independent action on the part of the State be suspended for a season, but that the attention of Congress be especially called to the importance of fixing the standard of Weights and Measures.

Before long, Congress had taken the much needed action, and established uniform national standards, probably the best attainable without the introduction of new names and ideas; and before the expiration of a single decade from the date of this report, Mr. Bache himself occupied the position of Superintendent of Weights and Measures for the United States.

Like the great body of scientific investigators throughout the world, he felt the convenience and many advantages of the metric system, and fervently looked forward to the time when that uniformity, for which the individual States of America and of continental Europe were then earnestly striving, should find its application on a grander scale, and when all civilized nations, scorning the prejudices which rivers, oceans or mountains might hem in, should join in one measure, one weight, and one coin; deeming this apparently far off, yet not impossible, attainment to be not merely an efficient source of immediate benefit, but also a great step toward hastening on that happy era, sung by poets and foretold by prophets,—

> "When the war-drum throbs no longer, and the battle-flags are furled
> In the parliament of man, the Federation of the world."

The only hinderance to his immediate advocacy of the metric system arose from a fear that the nation was not yet ripe for it, that new names would alarm the illiterate, and that premature efforts for its introduction might defeat their own end. Still he retained the meter as the fundamental unit for all the measurements of the Coast Survey; and in all discussion and official action, whether in the United States Office of Weights and Measures, in his many relations to the Mint, or as Chairman of the Committees upon Weights and Measures in this Association and in the National Academy, his policy always had reference to that desirable epoch, which must surely come, when the metric system shall be thoroughly introduced into the United States. How would his heart have swelled with joy could he have foreseen that, even now, while I speak to you, this has been established as the standard of Weights and Measures for more than half, and legally authorized for two-thirds of the population of continental Europe; adopted by one-half of that of South America; that the United States have not only put it on the same legal footing as the old systems, but have introduced it into their domestic and foreign postal arrangements, and are issuing coins of metric dimensions and weight, for the purpose of disseminating among our people an acquaintance with the system; that Russia has declared her readiness to follow, when the United States shall have also joined the long list of nations who have established it in good earnest; that China, at once the eldest and the youngest of the family of nations, is on the eve of establishing the same standards; and that, even in conservative England, its use has been authorized by statutory enactment; so that the elegant and harmonious system of meters and grams now stands in the same legal category with avoirdupois, troy and apothecaries weight, with beer-measure and wine-measure, cords, acres, stones, quarters of various sorts, and all the other arbitrary and incongruous denominations, which nothing but hereditary acquisition could save from the verdict of utter absurdity; divided too, as they are, into parts of 3, 4, 5½, 6, 8, 12, 14, 16, 31½, or whatever else they may be, certainly not decimal.

Two very extended investigations, in which Mr. Bache bore an important part, ought not to be omitted from mention here. The first is the well-known and elaborate Report upon Water Power, by a committee of the Franklin Institute, a document prepared by Mr. Bache, although he took no part in the published portion of the experiments. The other is the Report made in 1836, on the Explosions of Steam Boilers, which cost him some years of hard work, and is signed by him alone in behalf of the committee of the Franklin Institute. It is elaborate and learned, and rendered essential service in promoting judicious legislation. The causes of explosions are considered under five general divisions, comprising all the causes at that time recognized. Yet it is a most instructive consideration for the student that we are now, after the lapse of thirty years, aware that one of the most serious sources of danger arises from a cause at that time unknown: namely, the sudden evolution of steam in consequence of temporary diminution of pressure, and, perhaps, the mechanical agitation, produced by lifting the safety-valve,—the pressure of this steam, when disengaged, surpassing that which had previously sufficed to confine it in the liquid form. Happily, this source of danger also has now been obviated, by means of a principle closely analogous to that, which at this time, formed the subject of another of Mr. Bache's investigations, and regarding which his experiments led to an unfavorable report: namely, the principle of circulation in the water of a boiler. The celebrated Jacob Perkins, seizing, with the quick perception of genius, upon this principle, which had long been employed in the bleacheries of continental Europe, undertook, by introducing into the boiler an inner lining, open at the bottom, to confine the generation of steam to the annular space between the boiler-wall and this false lining, thus causing an upward current within this space, maintained by the water descending through the central core. The theory seemed good, but Bache's experiments showed that, far from greatly promoting the generation of steam, such small influence as was exerted by the inner cylinder, was in the opposite direction.

Now that we have the additional knowledge of thirty years on the subject, we easily perceive the reason. The water foamed in the annular space, not because it did, but because it did not, circulate freely. Had the water in the inner vessel been unable to receive any heat which had not traversed the outer one, and had the ratio of the sectional area of the inner vessel to that of the annular space been twelve or fifteen times greater than it was, a rapid circulation would have been established; foaming would have disappeared; the rapidity of formation of steam would have been increased three fold; and the water would have remained in such a condition that no diminution of incumbent pressure could occasion a sudden evolution of steam.

In the same year he made a thorough survey of the track and the effects of a severe tornado which had visited the vicinity of the city of New Brunswick, and obtained quantitative, as well as qualitative, determinations of its effects. The results here, as, likewise, those of similar subsequent surveys of tornadoes near Newark and near Philadelphia, tended to confirm the then new and unpopular theory of Mr. Espy, of an inward motion near the surface of the earth.

But I am dwelling far too long upon the manifold labors of this preliminary period of Bache's scientific career. To present an analysis of his labors during those seven years, would be an agreeable task; but far more than a proper portion of this discourse has already been demanded by the variety of their scope. Ingenuity, mental activity, interest in all branches of the physical science of the day, and a zealous industry that shunned no toil, characterize this early period of his career, as emphatically as that of his maturer life. Six chemical papers lie before me; among them are an analysis of anthracite coals, made jointly with Professor H. D. Rogers; experiments on the corrosion of metals by salt water; on the action of different alkalies in conferring hydraulic properties upon lime, and on a new method in alkalimetry. Here, too, are nine others, yet unmentioned, upon subjects in thermics and meteorology; among them, three historical notes, 1st, on the date of Dr. Franklin's discovery that our north-east storms begin at the south-west, fixing this date satisfactorily in 1743; 2d, on the received hypothesis to explain the greater quantity of rain which falls at the surface of the ground than above it; and 3d, on the discovery of the non-conducting power of ice; the origin of both these latter being conclusively traced to his great ancestor. There is a charming little paper describing experimental illustrations of various phenomena in thermics. There are methods of recording the force and direction of the wind, subsequently introduced with benefit into the Coast Survey. There are meteorological observations, and observations of meteors, simultaneously with Mr. Espy, for determining their height, and with Professors Henry and Alexander, for determining longitude, these last being the first successful ones made for this purpose; and there is one of the most noted of all his memoirs, the investigation of the Influence of Color upon the Absorption and Radiation of non-luminous Heat.

The basis of these experiments was the principle, that for each substance there is a thickness beyond which radiation from it is constant, so that their radiating powers are rightly compared, not by equalizing either their thickness or their weight, but by determining, for each substance, what the thickness in question is. Obtaining then a variety of pigments, of the same color differing chemically, and of kindred chemical composition differing in color, he undertook to determine their different powers of absorption and radiation; as, also, the modification of these powers, which changes of color, occasioned by chemical means, would produce. The principle in question was established, but no connection between color and radiation was detected.

I pass from this interesting period of Bache's life with an allusion to one more of his works, at that period eminently characteristic of the man, his polemical discussion with Olmsted regarding the periodical recurrence of meteors. Mr. Bache maintained that there was no recurrence in 1834; Professor Olmsted, on the other hand, maintained the reverse. Professor Bache instituted special inquiries at the military posts, where of course sentinels were on duty, along all the frontiers of the United States; also among the night-police of various cities, and at the Universities; and he found but one exception to the statement that no unusual number of meteors was seen. Of this controversy Bache wrote, in 1846:

"There is something yet to be found out on this subject, which may reconcile our opinions. Neither I, nor any of those watching with me, or for me, have seen an unusual number of meteors on the night of the 12th of November, in any year since the great night at Philadelphia, and we have taken great pains to be sure. Yet, I cannot doubt the testimony as given for some other places. . . . I had a complimentary letter from the Professor in regard to my manner of conducting the controversy, which I valued more highly than if I had gained the victory."

That "something yet to be found out" we now know, thanks to our colleague, Professor Newton, and to the brilliant celestial confirmation of the correctness of his computations on the night of November 13–14, 1866 and 1867. It affords the anticipated reconciliation of the two opinions, while the history of that controversy illustrates the firmness, mildness, charity and courtesy of our lost friend and leader.

Thus earnestly, variedly and successfully prosecuting his physical investigations, surrounded by a genial circle of friends, whose companionship he always loved to recall, enjoying the affection of his pupils in the University, filling an important position in the management of the Philosophical Society and of the Franklin Institute, winning for himself an honored name on both sides the Atlantic, dwelling with his kindred, their stay and staff,—his busy life went on, till suddenly a new path of usefulness opened before him, and led to an interruption of the whole tenor of his thoughts and ways.

Stephen Girard had died in 1832, leaving an immense sum of money for the endowment of a college for orphans. In 1833, Professor Bache had been elected one of the Trustees of the college, and on his thirtieth birthday, July 19, 1836, he was, by the unanimous vote of the other Trustees, appointed President of the Institution. On accepting this new position, so full of promise, he received instructions to visit Europe, in order to examine similar establishments there, and thus to procure the means of opening the college under the most favorable circumstances. Accordingly, resigning his post in the University of Pennsylvania, he prepared for an immediate departure, and sailed for Europe September 30, 1836.

During two years happily and profitably passed in visiting the principal countries of Europe, he formed many close friendships among the leading investigators of physical science, while actively prosecuting the especial objects of his mission; and moreover found opportunities for a determination of the magnetic dip and intensity at twenty-one European points, with the same apparatus and by the same methods which he had employed in America, thus rendering it possible to combine the results of both investigations, without danger of introducing extraneous errors. These objects once accomplished, he returned in October, 1838, in the hope and expectation of insuring an early opening of the college. During the winter of 1838–9, he prepared his "Report on Education in Europe," a large octavo book of 666 pages. Of the contents of this elaborate volume, I will not enter into any detailed analysis before the Association. By the universal consent of those best competent to judge, it enjoys the reputation of a master-piece. The fruits of an examination of about 280 schools, comprising examples of all the principal educational institutions below the grade of Universities in Great Britain, France, Switzerland, Holland, Italy, and the several German States, are here systematically presented; though the work claims only to be an abstract of the more prominent parts of the mass of documentary matter brought home by Mr. Bache, and arranged under his direction, so that, to use his own words, it might "always throw the light of experiment upon doubtful points in the working of our system of organization. In many cases the documents descriptive of the schools give the actual results of suggestions contained in the works on education."

The history of the delays in the opening of the Girard College, of the vast sums and almost interminable time expended on the construction of the building, and of the various other difficulties, which seemed full of unhappy auguries, needs no repetition here. No effort was spared by Professor Bache to accelerate the opening of the institution. We have seen that he hastened home immediately on the completion of his labors in Europe, notwithstanding the cordial recommendation of the Trustees, that he should remain. At the beginning of his report he says:

> "It is almost needless to say, that I am now not only ready, but anxious to render available as soon as possible, in the organization of the Girard College, the knowledge which has thus been acquired. A due execution of the instructions of the scholastic committee required not merely an examination of orphan houses and elementary schools, but of the various modes of education and grades of instruction. . . . I must be allowed to say, that in the course of attempting its execution, I have spared no personal exertion; and though I may regret it was not in abler hands, my conscience acquits me of having wasted any parts of the time and means so liberally placed at my disposal by my fellow-citizens."

A year passed by; the imposing structure, in which the orphans were to receive their education, remained unfinished; Professor Bache's salary continued; but there was no work for him to do. His mind was filled with the ideas and experiences on educational matters, which had accumulated during three years of continuous application to inquiries in this direction. Being therefore alike unwilling, by resignation of his office, to abandon the bright prospects which his enthusiasm had portrayed for the future of the college, or to receive a salary for which he was rendering no manifest equivalent, he conceived the idea of offering his services gratuitously to the city of Philadelphia, for the purpose of reorganizing its High School, then lamentably deficient in many important respects. His offer was accepted, and he received and assumed the position of Principal of the High School, which he retained for more than a year, effecting during that period a complete reformation of the school, which soon assumed position among the best in the land. Finding then that the college was still not in a condition to promise any speedy commencement of its activity, he insisted on resigning his salary, although against the remonstrances of the Trustees. He retained, however, the nominal office of President, and held himself ready to assume its active duties, whenever opportunity should offer. Meanwhile he received from the civic authorities the joint position of Principal of the High School and Superintendent of Public Schools; together with a salary which, though but little more than half that which he resigned, was a liberal one for those days. This position he retained for nearly two years, until in July, 1842, three years and a half after his return from Europe, he was reappointed to his old position in the University, and left the city-schools. His reoccupation of the Professorship was, however, of short duration, for in November, 1843, he received the appointment of Superintendent of the Coast-Survey, and here, at the age of thirty-seven years, began the great and crowning work of his life.

During the five years since his return from Europe, though his time and energies were greatly drawn upon by the exigencies of the educational duties, his scientific labors had been by no means intermitted. He had published his observations of the magnetic dip and intensity at twenty-one stations in Europe. He had organized a magnetic observatory at the Girard College, and raised the means for the prosecution of observations during a series of years, which demonstrated for the first time that the small changes of magnetic declination in Europe and America are not similar. He had invented an ingenious instrument for determining the dew-point, which is even now probably the best one available for that large class of cases, where the observations must be made by men not specially trained to scientific investigation. A series of comparative observations, with this instrument and the wet-bulb hygrometer, for about two years, is among the records of the Girard College Observatory. Long afterwards, Bache found that Belli of Milan had anticipated him in the general principle upon which the instrument depends. "The form of my instrument," said Bache, "is quite different from his, and I think it has advantages enough to entitle me to keep the name, but alas, principle was the soul, and I do not care much to dispute for the dead body."

The principle consists in cooling one end of a conductor below the dew-point, while the other is at the temperature of the air. The intermediate parts then assume different temperatures, and some one section will have the precise temperature of the dew-point. The conductor is quicksilver, contained in a highly polished metallic tube, on which the marginal line of deposition of dew becomes sharply marked. A thermometer, plunged in the mercury at this point, indicates, of course, the degree of temperature sought. The details of contrivance in this instrument are admirably managed; and, although doubtless the neat device of Regnault for attaining the same result, by the evaporation of ether from a thimble, furnishes a more accurate method in the hands of a philosopher, it may be doubted whether, in those of a mechanical observer, it would be found preferable.

During the same period he had introduced a modification of Osler's anemometer, avoiding the friction of the apparatus necessary for guiding the spring, as also the exposure of the spring to the weather and to great fluctuations of temperature. He had made a series of numerous experiments with Fourier's thermoscope of contact, in order to determine the conducting powers of powders and tissues, hoping thus to remove the obstacles which had prevented the farther prosecution of the subject of his first published paper, that on the inflammation of phosphorus. But finding these results unsatisfactory, he had invented a new thermoscope of contact better adapted to the purpose, and capable, by a slight modification, of being applied to a determination of the conducting powers of liquids.

And, besides these, he had carried on his investigations upon heat to a very considerable extent, and although the results were not, in his opinion, sufficiently definite and complete to justify their publication, still his expressions of hope that he might yet resume and complete these researches were ever earnest.

I cannot refrain from making known the reason of their discontinuance.

One room on the sunny side of his house was appropriated to these experiments; the various thermoscopes and all the subsidiary apparatus were arranged there, and the apartment was held sacred to scientific investigation. One evening, while he was attending a session of the Philosophical Society, an alarm of fire broke out in the neighborhood. His mother, then a member of his family, heard the alarm, and hastily entered the room without a lamp, to look from the front window. A crash reminded her, too late, of the inconsiderateness of her movements. The apparatus was entirely destroyed, and the first words which greeted her son on his return told him what he had lost. He made no reply, but went to the room and silently surveyed it. The destruction was complete, and the hard labors of nearly a year were rendered fruitless. An eye-witness has described it to me. He stood white with emotion for a few moments; then, turning away, only trusted himself to say that he would return soon, and hurried out of the house. Half an hour in the open air restored him to himself; returning, he consoled his mother, and made light of the occurrence; nor did he ever afterwards explain the reason why his observations on heat were discontinued.

It was in November, 1843, that Mr. Bache was appointed to the Superintendence of the Coast-Survey, his appointment to the Office of Weights and Measures following, a month later. The volume of testimonials and recommendations, upon the strength of which this appointment was made, has been shown me; and their number and character has made a deep impression. I cannot believe that such a weight of recommendation was ever brought at any time in support of a candidate for office, on purely intellectual grounds. I can think of no man in the country, eminent in physical science or holding a prominent scientific position, whose name was not signed to some one of that voluminous mass of memorials, asking the appointment of Professor Bache. All the scientific societies and colleges, together with several of the learned associations of Europe, gave their influence and added their indorsement to the request. How such a unanimous declaration on the part of experts could have failed of success, it were difficult to conceive; assuredly it was tenfold more honorable than any mere appointment. But to all these was added a yet more effective influence, growing out of the personal and political relations which by good fortune were brought to bear, and which it was impossible to resist. The President, Mr. Tyler, issued the commission, in spite of the avowed and vigorous opposition of the Secretary of the Treasury, Mr. John C. Spencer, under whose immediate direction the Coast-Survey belonged.

So far as the earnest and unanimous support of the lovers of science in the country was concerned, (science had then few, if any, real votaries other than those who were incidentally enabled to prosecute their studies in the intervals of other professional labor,) Mr. Bache entered upon his new duties with confidence. But he found his position one in which such moral support could afford but little aid. Internal disorganization, insubordination and dissension embarrassed him on the one hand; and the distrust and unconcealed animosity of the Secretary shackled him on the other. These were circumstances to evoke and make manifest the true greatness of the man.

Mr. Hassler, his predecessor, was a man of high attainments and ability, whose scientific management of the work, which he had himself initiated, had won universal approbation. He had emigrated to this country from Switzerland at the very beginning of the century, and had brought with him ideas of scientific accuracy and thoroughness, which the public mind in America was not yet sufficiently enlightened to appreciate, or even to understand. He gave to the Survey the chief energies of his life, and, undeterred by its suspension for nearly fifteen years, resumed its prosecution, when permitted anew, with the same zeal which had marked its inception. In Mr. Bache's words, "For his successful struggle against great difficulties, his adopted country will, no doubt, honor his memory, as the pioneer in a useful national undertaking." On the other hand, he was a man of great eccentricity of manner, and not endowed with administrative ability. Science was at a low ebb among Americans when he came to our shores, and but few received the benefit of the scientific knowledge which he brought with him. Excepting Nicollet, himself a foreigner, and Dr. Patterson of Philadelphia, Hassler appears to have little acquaintance, and still less personal intercourse, with learned men in the United States. The scientific development of the country seems to have gone on unperceived by him; and, to quote the words of one who knew him, "he died in the belief that the nation as a whole, was, in 1843, where he had found it in 1801, so far as its science was concerned." Comparatively few native Americans obtained employment on the Survey under him, and one of his surveying party has informed me that he himself was the only man of that party who spoke English habitually. His scientific affiliations were exclusively transAtlantic, and while he seems to have entertained a sort of general affection for his adopted country, he apparently looked upon Americans as necessarily tyros in scientific matters, and deemed the refinements and elegancies of the higher Geodesy matters entirely beyond their comprehension. When on one occasion he had applied for an increase of salary, the Secretary of the Treasury, upon whom he was urging his claim, is said to have replied, "Why, Mr. Hassler, that is as much as I receive myself;" to which with charming frankness he replied, in strong Swiss accent, "What if it is! Any President can create a Secretary of the Treasury; but only the Almighty can make a Superintendent of the Coast-Survey."

"At the time of Hassler's death," I quote the language of a distinguished officer of that period, "the condition of the Coast Survey was anomalous and Ishmaelitish. Every man's hand was against his neighbor. Hon. John C. Spencer, the Secretary of the Treasury, was the real head of the Survey; and the principal assistants reported directly to him, and not to Mr. Hassler, who was thus reduced to the position of nominal Superintendent, but the real Chief only of the primary triangulation party."

Mr. Spencer was, as I have said, strongly opposed to Mr.

Bache's appointment. He favored the claims of the senior assistant. The two assistants, next in order of appointment advocated the same, desiring to see the principle of succession by seniority established. Other assistants urged yet other men, but none favored Mr. Bache. After his appointment, four of the eight yielded their coöperation, as did a fifth after a time, but the others were as ingenious as they were active, in devices for establishing and maintaining a system of petty persecution. Never was magnanimity more grandly exhibited than in Mr. Bache's course, while making himself Superintendent in fact as well as in name; but on this there is no need to dwell. Illustrations of his greatness of spirit are wanting at no period of his career.

Perhaps I may best illustrate the conciliatory power of Mr. Bache, and the magnanimity of Mr. Spencer, by reading to you a letter, written by the latter at the time of his resignation, being only five months after Mr. Bache had taken charge of the Survey in opposition to his wishes. It is addressed to Professor Bache, and forms a fitting pendant to the other letter, already laid before you.

TREASURY DEPARTMENT, May 1st, 1844.

Sir: — I am unwilling to leave this Department without communicating to you the great pleasure I have derived from the intercourse which has subsisted between us since your appointment as Superintendent of the Coast Survey; and my conviction of the great service you have already rendered the country in the arrangements made for carrying on that work. The system, order and regularity to which you have brought the complicated and difficult operations of that great work, afford the strongest assurance that it will now proceed with vigor and despatch, as well as with economy. My thorough knowledge of all your difficulties, plans and improvements, derived from the intimate communications that have been maintained between us, justify me in saying, that in my opinion the work could not be entrusted to more capable and judicious hands than yours. I shall look forward with great anxiety to the accomplishment of those great results which I am confidently anticipating in the successful prosecution of your very laborious and highly responsible undertaking.

With great respect and esteem,

Your friend and servant,

J. C. SPENCER.

Prof. A. D. BACHE,
Sup't Coast Survey.

In these five short months the moral and intellectual power of Bache had not only triumphed over the obstacles which the Secretary mentions, but over the numberless misrepresentations to which he had at first lent an ear; and finally over the Secretary himself, whose manliness forbade him to retire to private life without first leaving a public record to remedy any previous injustice. He had regarded and described Bache as a "mere college professor," without practical administrative ability, or any special qualification for the Coast-Survey. But he saw and acknowledged the error, and remained his fast friend until death.

Six months thus passed in establishing authority, and in learning the precise condition of the work which he had undertaken to prosecute, and regarding which the continued ill will of the three senior assistants and the chaotic condition of its affairs, had at first thwarted his endeavors.

But at last, freed from the influences of internal disorder and dissension,—having gained the confidence of the Treasury Department, having established discipline with the three insubordinate assistants, without any manifestation of unkindness on his own part, and having secured the respect and cordial regard of the other officers of the Survey,—Professor Bache was able to apply his energies with effect to the development and prosecution of the great undertaking before him.

The first step was to enlarge the scope of the work, avoid sectional jealousies, and accelerate its execution, by dividing the coast into sections, in each of which a base-line should be measured and the triangulation extended in opposite directions from this base to the confines of the section, where the work of the adjacent one would meet it; each thus testing the other. The advantages thus gained in soothing prejudices, already serious in those States which the connected primary triangulation of his predecessor had not yet reached, was greater than I can describe, and conduced in no small measure to the public favor and support, which his tact and discretion, even more than his scientific power, soon won for the Coast-Survey. Within the first year, the operations of the Survey were carried on in nine States of the Union; within the second, in twelve; within the third, in fourteen, notwithstanding the withdrawal of all the army officers, on account of the Mexican war; and within the fourth year in seventeen,—the Atlantic and Gulf coast having been divided into nine sections, a division still retained.

The first season was in a great degree devoted to the trial of instruments and methods. The thirty-inch theodolite was, and is to this day, a truly serviceable instrument, but the astronomical apparatus belonged to a by-gone era. To Lieutenant T. J. Lee, and to Mr. Boutelle, both of them gentlemen of experience in practical observation, was assigned the duty of testing the instruments available for latitude-determinations, the chief of which was a Borda's repeating Circle. After a summer of faithful and repeated trials they reported that their best results for latitude had been obtained with the vertical circle of a six-inch Gambey theodolite, which they had borrowed. There were three transit-instruments, two of them five feet long, and the third and best of the three, only two feet. All proved unequal to the demands of geodetic astronomy in 1844, and were soon replaced by instruments of more modern construction. Observations of the dip and variation of the needle, and of the intensity of terrestrial magnetism, were introduced as part of the regular routine. So too were observations of the tides. The employment of solar observations for determining azimuths was exchanged for observations of the pole-star at both elongations. The form of geodetic signals invented by Mr. Borden, and used by him in the trigonometrical survey of Massachusetts, was substituted for that previously used; and is still regarded, by common consent, as the most accurate and convenient form ever devised.

As rapidly as means allowed, the services of American scientists throughout the land were enlisted in aid of the Survey, and the whole intellectual resources of the country thus made tributary to its usefulness and success. Thus Walker, Peirce, Bailey, Agassiz, Barnard, Kendall, Mitchell, Bond, Alexander, and many others were called on to assist in the advancement of the undertaking; and this large and wise policy prevailed during the whole period of his superintendence. Various other institutions took the name "national," but there was only one really national scientific institution for the first twenty years of his administration, and that was the United States Coast-Survey.

As the work gradually expanded under his guidance, many of the ablest officers of the Army and Navy were brought into the Coast-Survey service. They found an opportunity for the use and development of their powers which otherwise might long have remained wanting, and gladly served under a civilian who manifested such a knowledge of men, and such a capacity for using them. It is surprising to see how many of our ablest military and naval officers found employment, and attained distinction, in the Coast-Survey. Of the army, I need only mention the present Chief of the Engineer-Corps, General Humphreys, the worthy follower of Totten, Generals Stevens, Foster, Prince, Cullum, Cram, Hunt,—so too Johnston and Hill, whose intellectual powers were then enlisted in their country's service. Of the navy, among others,—Vice Admiral Porter, the two Rodgerses, Davis, Gilliss, Sands, Craven, Ammen and Luce. During our late war, a large majority of the most useful commanding officers of the navy had served upon the Coast-Survey, and they did not hesitate to assert that their experience there had been of the highest service to them in the naval duties of the war.

In the second year of Bache's administration the base-apparatus was perfected by him, the Zenith Telescope first employed in the Coast-Survey for the determination of latitude, and the exploration of the Gulf-stream commenced. In the third, the magnetic telegraph was first introduced as a regular geodetic method for the determination of longitude, and a special party organized under Mr. Walker for the prosecution of longitude-operations. In the fourth year, a chronometic expedition was organized for the better determination of the transAtlantic longitude, and the method of star-signals adopted for telegraphic measurements,—a method "destined," to use Walker's own words to this Association, "sooner or later to perfect the geography of the globe." The system of magnetic and tidal observations had then attained an extended development; and arrangements were already in progress for the measurement of two arcs of the meridian, and one of a parallel of latitude.

I will not undertake any detailed narration of the successive stages in the development of the Survey under Professor Bache; but a few words may be deemed not amiss regarding some of the more marked steps in its progress. The apparatus for the measurement of base-lines, devised by Professor Bache in 1845, and constructed by Würdemann, whose services had even at that early day been secured for the Coast-Survey, and to whose exquisite skill and ingenuity many of the details are due, attained a degree of excellence before unknown. It can be safely asserted that with this apparatus, under Bache's skilful manipulation, the practical sources of error became less than the theoretical, or, in other words, that a line could be measured with such precision that the uncertainty arising from all errors of every kind, incurred in the measurement, was less than the uncertainty in the length of the standard of measurement. This wondrous accuracy was attained by the introduction of two new principles, both due to Professor Bache.

Owing to differences of specific heat and conducting power, different metals subjected to the same circumstances, do not change their temperatures with equal rapidity. Consequently an apparatus constructed for perfect compensation by the joint employment of brass and iron, on the principle of the gridiron pendulum, may not be correctly compensated, so as to remain of unvarying length during a change of temperature, which is in fact the very condition which naturally exists at the time of use. By numerous experiments Bache succeeded in so arranging the cross-sections of the bars, that while the extent of their surfaces remained the same, their masses should be inversely as their specific heats, allowance being made for their conducting powers. The same varnish being applied to the surface of each, in order to confer equal absorbent capacity, the compound bar must then retain the same length, not merely at all temperatures, but during all changes of temperature.

The other principle which he introduced consists in the substitution of actual for optical contact, thus dispensing with the use of microscopes, a practical gain of great value in the field. The contact-level of Bessel finds here an appropriate application; the agate plane, carried indirectly by the compensation lever which connects the brass and iron at their free ends, abuts against an upright lever on the other bar, which transmits the pressure to the contact-level.

A description of this exquisite apparatus was given to this

Association at its Washington meeting by our lamented colleague, the late Major Hunt of the United States Engineers. At Bodie's Island in North Carolina, Professor Bache measured a base-line six and three-fourths miles long, in ten working days, with a total probable error of less than one tenth of an inch. On one day at this time, 1692 meters, or one and a sixteenth miles, were measured in eight and a half hours. In measuring the base on Epping Plains in Maine, 8716 meters long, the probable error committed was less than sixteen millimeters; and the probable error of any distance in the primary triangulation of the New England States is to its total length, as 1 to 288000.

The determination of latitudes with the Zenith Telescope by Talcott's method was first tested in 1845, and proved so far superior to any other known means of determination possible in the field, that it was adopted by Professor Bache for the Coast-Survey operations. I need not tell you of the excellences and precision of this method, for it is now more than twenty years since the attention of astronomers was first called to it, while the annual Reports of the Coast-Survey, and numerous communications to scientific periodicals in this country, have furnished continual illustration of its preëminent merit. And in a recent number of the Proceedings of one of the leading scientific societies of Europe, published in this very year, 1868, attention is called to the Coast-Survey method as a very excellent one, and well deserving of a trial! Here, too, as with the base-apparatus, a degree of accuracy is attainable in observation, surpassing that afforded by the fundamental data; so that not the errors of instrument or observer, but those of the star-declinations, limit the correctness of our results. To Bache we owe the recognition and adoption of this transcendent method, and to him also those refinements of process and improvements of apparatus, by which alone its accuracy is rendered possible.

To the telegraphic determination of longitudes, and the development of the method of star-signals, I have already alluded. The latter was soon found applicable, with almost equal advantage, to the regular observation of transits in fixed observatories, and all the apparatus devised for the one purpose answered with equal aptitude for the other.

The simple and easy process of making telegraphic comparisons of clocks, whose respective errors are determined by local observation, was early discarded in spite of its tempting facility, since the apparent accordance of results thus obtained soon showed itself illusive. Bache's rule in the Coast-Survey was that all the scientific work should be executed in the most thorough and accurate manner which the resources of science and art would permit. He never shunned a tenfold labor, if it was to be repaid by double precision, accepting the great principle which prescribes a higher rate of effort as we climb to higher degrees of refinement. Nor did another great law which the history of science has taught from the beginning, and is teaching now, fail of its continual application and illustration; just as it will not fail hereafter, unless all inferences from experience are futile. This law ordains that the conscientious investigation of truth for its own sake shall be rewarded by some unforeseen practical benefit. The struggle to attain, at whatever effort it may cost, all possible accuracy throughout those investigations whose value is dependent on their precision, finds unexpected recompense in new clues to phenomena before unknown, and in the disclosure of new laws. Thus it was that the method of star-signals led to the method of telegraphic registration, now universal in observatories.

This important advance in practical astronomy received its full development in the Coast-Survey. There, long years ago, its methods were brought to the same degree of refinement and completeness of detail, which they have but recently attained in Germany. The precautions requisite in observing, the study of personal equations both in noting the transits and in reading the records, the modes by which the clock can best be made to graduate the time-scale, the various forms of chronograph for maintaining that time-scale,—all sprang from the Coast-Survey, and received their full development either from the regular offices of the Survey, or from others acting temporarily as assistants. Among the earliest results of this method as applied to the measure of longitudes were the discovery that the time required for the transmission of signals by electric telegraph was appreciable, and therefore measurable, and the determination of their velocity. These investigations and results were of course not all of them due to Bache directly or alone, but none the less were they due to him. Not merely to the general expansion which he gave to the operations under his direction; not merely to the large policy, according to which he secured astronomers for astronomical, physicists for physical, and mathematicians for mathematical, research. These would indeed have been ample grounds on which to claim for him a large share of the honor due for valuable discoveries and successful applications. But in fact his personal relation to the results was far closer. He knew, understood and guided, even when one or more other minds were active between him and the results. Accomplished himself in practical observation, skilful in experiment, thoroughly acquainted with the progress of all the larger investigations prosecuted by others under his direction,—he was no administrative officer in that too frequent employment of the term, which dispenses with the understanding of work directed to be done by others, and actually makes the words "administrative" and "executive" to denote opposite qualities. Day by day, step by step, in the successive detection of principles, elaboration of methods, discovery of facts, he was in constant intercourse with the investigator,—stimulating his zeal, encouraging his hope, suggesting new ideas or infusing needful caution. Yet with unfailing magnanimity he never claimed in such cases any personal honor for success, nor did he disown any personal responsibility for failure. May I be allowed to repeat a sentence which I addressed to this Association fifteen long years ago, when we mourned together over the loss of Walker, and you had called on me to recount his honorable deeds? In speaking of the method of star-signals, I then said:

> "Mr. Walker has informed me that this suggestion was due to the Superintendent of the Survey, but its practical application seems to have been a result in the elaboration of which the two bore an equal part. At least I may be permitted to state the still more honorable fact, that in the very many conversations which it has been my privilege to hold with each of the two gentlemen separately upon this interesting question, their descriptions varied but in one salient point, namely, that each ascribed the chief merit to the other."

Thus by the use of the Zenith Telescope, combined with the determination of longitudes from the adopted meridian by the exchange of star-signals, the geographical position of the primary astronomical stations of the Survey could claim, ten or fifteen years ago, to be determined with more accuracy than that of any European observatory.

The temptation is strong to recount the similar advances made through Bache's administration of the Coast-Survey, in almost every department of physical science. Not only Geodesy and Astronomy were thus made gainers by new methods and implements of research, but other sciences were similarly promoted and their advancement stimulated.

Stations for tidal observation were established all along the Atlantic, Gulf and Pacific coasts. Self-registering tide-gauges (the invention of the ingenious Saxton, whose invaluable services Bache early secured, and retained to the last), were brought into extensive use. Our knowledge of tidal phenomena and laws assumed new proportions, and by unwearying persistence in questioning, the secret of many a mystery in their complicated action was extorted. The character of the ocean currents along our coasts was determined, and their causes elicited. All along the shores of the Atlantic, Pacific and Gulf of Mexico, beyond the lines of soundings, the deep-sea lead, and the deep-sea thermometer were busily plied by the exploring vessels of the Survey, while the immediate coast was fringed with a net-work of soundings,—the deep-sea lead, thermometer and sounding lines all being at the same time essentially improved. Twice was Agassiz sent to study the coral reefs of Florida, to discover the method of their formation and the laws which promote and restrict their growth. The most accomplished students of the infusoria were kept supplied with specimens from the dredge and sounding line for their microscopes. The magnetic instruments were improved, and the magnetic constants determined for every important point possible, within the reach of the Survey.

The exploration of the Gulf-stream, commenced in 1844, was vigorously prosecuted, its temperature at the several depths determined, and its structure and laws for the first time detected. The cold wall of water between the Gulf-stream and the shore, as also the division of the stream proper into alternate bands of warmer and cooler water, were discovered, measured and mapped out for the benefit of navigators and the use of scientists. So too were a series of nearly parallel ranges of hills at the bottom of the stream, the most westerly being 1500 feet high, and the whole forming a series of corrugations corresponding with the bands of warm and cold water. The facts that the general direction of the stream is governed by the configuration of the bottom and that the temperature varies from year to year, thus standing in an important relation to the meteorological phenomena of the coast, were also detected. These discoveries were, in a large degree, made by Mr. Bache individually, upon a study of the observations, although no small share in them justly belongs to Professor Trowbridge. In the year 1860, fourteen sections of the stream had been surveyed, 300 positions upon them occupied, and 3600 observations of temperature made.*

But I must not continue in this strain. That Bache touched no question of science which he did not adorn with new discoveries and new means of attaining yet farther ones,—that the range of the Survey was made to cover almost the whole range of physical science, from the structure of the microscopic dwellers in the bed of the ocean, up to the improvement of

*During Professor Bache's Superintendence of the Coast-Survey, the positions of 9816 geodetic stations were trigonometrically determined.

4989 triangulation-stations were occupied.
267 magnetic-stations were occupied.
68 base-lines were measured, being 150 statute miles in total length.
102 longitudes were astronomically determined.
151 latitudes " " "
82 azimuths " " "
12850 square miles of area were topographically surveyed.
38850 " " " " " hydrographically "
7222449 soundings were taken.
7500 specimens of bottom were obtained and preserved.
1030 tidal stations were occupied.
133 shoals and reefs were discovered and determined.
41 important channels were discovered and their changes determined.
1020 current stations were occupied.
970 topographical snap-sheets were constructed.
877 hydrographical " " "
270222 sheets of charts were printed.

In addition to these the positions of many hundreds of isolated rocks, ledges and shoals were for the first time accurately determined.

The party directed by Professor Bache in person occupied, during the same period, 49 primary triangulation-stations, measured 6 base-lines, of which the aggregate length was 33 statute miles, and determined astronomically 5 longitudes, 29 latitudes, 31 azimuths.

lunar tables and the determination of positions of fundamental stars,—you all well know.

I have said that to him the scientific progress of the nation is indebted, more than to any other man who has trod her soil. Nor was this bold statement made in forgetfulness or ignorance of the great debt we owe to many illustrious men. Nor do I fear that any of you who know his services will think it overstrained or ill-considered. I call you to witness that it is true; that not his great ancestor who outvied Prometheus, not the statesmen who have guided the legislation of the republic, not all the educators, who have shaped the policy of its colleges and schools, not all the great masters in physics or mathematics, zoölogy or geology, have so effectively, widely or intensely stimulated the advancement of science in America, as Bache did through the agency of the Coast-Survey. What was the branch of physical science, which was not called upon to minister to the Survey, or which failed to receive an impulse from it in return? Had the chief object of this work been the promotion of scientific research, it could not have been more effectively aimed at, or attained.

Addressing a scientific body on the scientific career of our departed leader, it is only from this especial point of view that I have considered his administration. But that would be a very one-sided presentation of the facts, and one eminently unjust to his memory, which should leave on a single mind the impression that the scientific researches or influences, of which I have spoken, were attained at the sacrifice, or disregard in the slightest degree, of those important practical purposes for which the Coast-Survey was established and supported. So too would it be unpardonable to omit, from the recital of his services and achievements, the work he did for commerce, for the national defence, and for the development of national resources. All this he accomplished the more thoroughly and the more economically, in that he called to his aid the resources of the highest science. Although embarrassed by difficulties peculiar to our own country and offset by few corresponding advantages, his survey of the Coast of the United States may challenge comparison, as regards its completeness and thoroughness, with any geodetic or hydrographic work ever executed. The ocean currents have been explored with untold gain to our seamen, the variation of the needle, and its rate of change, determined with unsurpassed precision along the three seaboards of our country, the tidal laws so well determined that the tide-tables published by the Survey for many years have been found conformable with observation to a degree previously unattained in any part of the world, the heights of mountains and the depths of oceans measured and mapped out,—and all this accomplished at a cost inferior, almost beyond comparison, to that of similar surveys in other countries. As has been well said by Professor Trowbridge, the annual cost of the Survey, at the time of its most extended activity under Mr. Bache, was but little more than the cost of a first-class steamship. It never reached an annual cost approaching the price of some of the floating palaces which ply upon Long Island Sound, or up the Hudson. Nor was there probably a single year of the twenty-three years of Bache's superintendence, when the discovery of reefs, shoals, currents, channels or rocks, would not have been cheaply purchased by the nation, at an outlay of all the survey ever cost. So much it may not be amiss to say upon this purely practical point, since, though of course needless for you, it might not be so for others to whose eyes these words may come.

But his benignant influence was not alone exerted through the agency of the Coast-Survey, with which his name is, and forever will be, associated. Can any of us forget his relation to this Association, of which he was one of the founders? No man surpassed him in efforts for its success. Never absent, never without some tangible proof of interest and good wishes, —his counsel, more than that of any one man, was sought on all questions of policy, and his opinion received with unsurpassed respect. Over three of the fifteen sessions of the Association held during his life, he presided, and hither he brought the annual contributions of his abundant discovery.

So was it also with the National Academy, the complement and coadjutor of this Association. There too he was a founder, the most active member, and its President. And but for the bitter dispensation which cut him off amid his usefulness and honor, his well-known voice would have sounded in your ears, calling on both institutions to stand firm by one another, mutually supporting, mutually dependent, and capable, by joint action and reciprocal confidence, of accomplishing fourfold what either could do alone.

So too was it with the Smithsonian Institution. From the beginning he was a Regent, and to his active support and earnest endeavor, more than to that of any other, we owe it that this Institution, under its present honored head, has been able to adopt and to maintain a policy, which shall make it what its founder intended: "an institution for the increase, and the diffusion of knowledge among men." It is safe to say that, but for Bache, Professor Henry would not have assumed the Secretaryship, nor have been willing to retain it; and but for Bache, his wise policy would in all probability have failed of full adoption.

No scientific undertaking, indeed, failed to receive from Mr. Bache hearty sympathy, personal assistance, and whenever circumstances warranted, official support. Gilliss's Expedition to Chile, Kane's and Hayes's Polar expeditions,—all of which, like many others on a minor scale, he served with his counsel, his influence, his official assistance and his purse,—illustrate his anxiety to aid the extension of human knowledge.

But it was not merely by his ardent love of science, and disinterested devotion to her welfare that he accomplished so much. His fertility of device, unconquerable assiduity, large policy, generous impulses, patriotic devotion, might well have coexisted without yielding such fruits in the development of the Coast-Survey, or such a mighty power for good in the promotion of science throughout the United States. More than these was needed; far more than these he possessed. It would be safe to say that the greatest of all his mental gifts or attainments were his marvelous knowledge of human nature and his unrivaled skill in using it. He had studied men, as he once expressed it to me, as he would study physical phenomena. To a faculty of persuading the most obstinate, of soothing the most irritable, of encouraging the most disheartened; to a power of stimulating the indolent, controlling the impulsive, winning over opponents by the charm of his manner, and confirming friends by the truthfulness and sincerity of his nature,

—he added that rare endowment, which imbued others unconsciously with his own zeal. His was what might be called a magnetic nature, for his companionship evoked latent aspirations, and pointed to noble aims. It was to his personal, more even than to his scientific, qualities, that the Coast-Survey owed the recognition of its importance by Congress and the people, and the annual provision made for its maintenance. He knew the secret of obtaining work from his subordinates, by doing more than they did. His desire to accomplish much himself led him during half the year into the field, where he in person performed the most difficult geodetic operations; thus insuring the accuracy of the portion accomplished by himself, and maintaining such an intimate acquaintance with the practical details as enabled him to understand the real needs of those engaged in similar operations elsewhere. Annually he made tours of inspection through all the sections accessible to him. He took a personal share in the details of all the explorations; and no assistant, permanent or temporary, felt, in entering on a new field, that his duties or powers were clear before him, till they had been thoroughly discussed with the Superintendent in person.

The qualities in which he manifested a high order of genius were especially those by which he governed men. Had his tastes been military or political, instead of scientific, none the less would he have been a Chief. It may safely be said that by no act of his life did he ever curtail any man's means of usefulness, or fail, whenever it was in his power, to render available whatever abilities might be disclosed. Justice and even-handed firmness controlled his action. No man was ever readier to acknowledge and atone for a wrong done by him, in thought or deed. Cautious in plan, bold in action, generous without impetuosity, as courteous and considerate of his subordinates as though they had been his superiors, ever as open to conviction as to argument, such was his noble character. With these traits was united a feminine tenderness of heart, and an intensely sympathetic nature. To him all came for comfort in personal sorrow, for sympathy in bereavement, for help in calamity. And his purse, like his heart, stood open to his friends, and to the needy.

In a critical analysis of his character, the traits which I have just mentioned would occupy the most prominent place, but with them the mention of one other quality would be imperative. I allude to his keen appreciation of humor, his love of pleasantry and jest, and his social geniality. These alleviated the cares of severe administrative duty, and the anxieties inseparable from his official position,—peculiarly great in our own form of government, where the needful appropriations are made by legislative bodies continually varying, and where the personal hostility of a single individual may be made the occasion for denunciation of a national work. Without his social qualities, and his power of banishing at will the oppression of grave cares, his precious life would scarcely have been spared one-half so long, as it was in fact vouchsafed us. This love of humor, so essential in a well balanced mind, was his in no stinted degree, and often was he the center of a group of delighted friends, whose enjoyment of his fun was scarcely exceeded by their reverence for his intellectual and moral worth.

His administration of the Coast-Survey was by no means an easy one in its political relations. For many years there was scarcely a session of Congress, without some vehement attack upon the Survey in each House, made for the purpose of defeating the appropriations. The causes of these were various, and many of them will occur to you without enumeration. Hostilities growing out of his original appointment and incapable of being allayed, jealousies on the part of other institutions professedly established for kindred purposes, resentments on the part of persons subjected to discipline, furnished fuel for the flame, in the form of ready assistance to whatever threatened the welfare of the Survey, come from what quarter it might. At one period, the representatives from inland districts, impelled by narrow local jealousy, opposed the expenditure of public money for purposes which they supposed not so useful to their own States as to others. At another time certain naval officers disseminated, to a considerable extent, the impression that, because it was a Coast which was the subject of Survey, the work should be placed in charge of nautical men. At still another date, similar claims were urged in behalf of the army. But the minds of the people, and of their legislators became enlightened, so that for some time before the recent war, the facts were recognized, that the services rendered were to no particular State so much as to the nation, and that the helpful aid of army and of navy were alike requisite, as well as that of a corps of civilians trained exclusively to the work.

One of the most violent of these organized attacks was made about the year 1849, and doubtless resulted in permanent good to the Survey, from the multitude and earnestness of the manifestations which it called forth from all parts of the United States, and from Europe. The learned Societies, the Insurance offices, Chambers of Commerce and Boards of Trade, throughout the land, hastened to send their testimony to the services which it was rendering, and their earnest protests against any interference. Men of the highest eminence throughout Europe joined in the appeal. In August, 1849, Schumacher wrote him:

"In an epoch like this, where everywhere associations are formed for material interests, it is high time that we also unite our forces, to protect the higher and purer scientific interests. . . . You will see by the inclosed letters of our common friends, Mr. Arago and Baron Humboldt, how anxious they are to know if the great work you have undertaken will remain in the hands to which the whole scientific world would have intrusted it. Let us hope that the fear they entertain is only founded upon vague rumors. Your country must be proud to call you her own, and will repay you in gratitude what you do for her scientific glory."

A month previous Arago had written to their common friend:

"There is a rumor here that Mr. Bache is threatened with losing the place he is filling in so distinguished a manner. I beg of you to give me information about it, so that we can join with all the most distinguished scientists of Europe to prevent so great a misfortune. The officers of the American navy are proverbial for their gallantry; they know perfectly well every part of the nautical art; still, I shall do them no wrong by stating that Mr. Bache stands higher than they by a hundred cubits in matters of Geodesy, and Physics of the Globe. And I will prove by numerous examples how very useful it is even for the truth of the results, that a man should be superior to the task intrusted to him."

So also Humboldt:

"You know better than I do in how high an estimation the Director of the Survey of the Coast stands, not only among us, but among all the most illustrious men, who in France and England are interested in the study of Geography and Nautical Astronomy. . . . In a region of the globe where the direction of oceanic currents, the difference of temperature produced by these currents and by the upheaval of the bottom, and the direction of the magnetic curves offer so important phenomena to navigators,—such a great work could not be better placed than in the hands of Dr. Bache. The government of the United States has acquired a new right to our gratitude by protecting nobly this that has already fixed the attention of the Hydrographers and Astronomers of Europe. I should be glad to think that in a country, where I am honored with so much regard, my feeble testimony might contribute to enliven the interest which is due to the excellent labors of Dr. Bache."

The President of the Royal Geographical Society of London, in his Annual Address to the Society in 1852, commented with expressions of strong admiration upon the practical management of the Survey, and characterized it as "one of the most perfect exemplifications of applied science of modern times." And six years later, Murchison, then President of the same Society, in his address presenting to him the great gold medal, used this language:

"Whether we regard the science, skill and zeal of the operators, the perfection of their instruments, the able manner in which the Superintendent has enlisted all modern improvements into his service, the care taken to have the observations accurately registered, his modest and unpretending demeanor, or the noble liberality of the Government, tempered with prudent economy, all unprejudiced persons must agree that the trigonometrical Survey of the United States of America stands without a superior."

Such expressions as these, uttered from all quarters at home and abroad whenever the work was menaced, speedily removed in every instance, all occasion for alarm to its friends.

The last serious attack was in 1858, when Bache's firm and unyielding defence of the scientific interests at stake in the well-known contest at the Dudley Observatory, was the occasion for the most virulent of all the onslaughts ever made upon him. At first it was supposed that by the inauguration of a regular system of attack upon the Survey, Bache would be frightened from his position at the Observatory. But where either moral principles or the interests of science were involved he knew no fear, and what was commenced as a means of alarm, was continued only in the spirit of revenge. The daily press was filled with attacks upon the Survey, its policy, its value and its Superintendent, some of them violent and some insidious. Pamphlets were prepared and sent to members of Congress at their homes, and to all prominent public men in Washington. Relations were opened with former opponents of the Survey, and with all who could be found, supposed to be disaffected towards it in any way. It is a source of peculiar regret that obvious circumstances preclude me from entering into many particulars and illustrations here, which others might have laid before you; and which would place in strong relief the magnanimity, the force, and the lofty chivalry of spirit in our lost leader. He stood like a rock, upon which the storm beat in vain.

> "O iron nerve, to true occasion true!
> O fallen, at length, that tower of strength
> Which stood four-square to all the winds that blew!"

It will forever be among my most precious memories that in that time of trial, it was my privilege to receive his approval from the beginning. The very last, the passive abandonment of the moral struggle, when physical force became involved, was the only step which failed of his full indorsement. This was to me the greatest pain of all; but happily dispersed, when upon farther knowledge of the circumstances, his opinion was changed, and his priceless approval extended to the whole of my course in that bitter contest.

A year later, at the Springfield meeting of this Association, a Report on the History and Progress of the American Coast-Survey, was presented by a committee of twenty men of science not connected with the Survey, and who had been appointed for the purpose at a previous meeting. Fortunately the attacks of the preceding year had failed of their effect, in consequence of the manifestly personal motives which inspired them, and of their evident connection with the Observatory at Albany. This able report, commenced by Judge Kane of Philadelphia, and after his death prepared by our honored late President, Dr. Barnard, then at the head of the University of Mississippi, presents a thorough analysis of the great work accomplished, and the strong language in its praise, with which the report concludes, received the unanimous indorsement of the Committee.

But this inadequate sketch of Bache's services would be yet more incomplete, were the record of his scientific achievements, during the last twenty years of his life, confined to the work officially done through the Coast-Survey, which he had made so essential to science in America.

His own investigations and discoveries during this period have not been enumerated, and though from the nature of the case they were mostly derived from data collected by the operations of the institution to which he devoted his energies, none the less were they the fruits of his own intellectual labor. I have endeavored to prepare a catalogue of his own memoirs,—a large proportion of which were presented to this Association, which he fondly cherished, and upon the sessions of which he was an unfailing attendant.

Most of these memoirs are upon subjects connected with the new methods which he introduced into the Survey, the measurement of bases, or the phenomena and laws of the tides, of the Gulf-stream, and of terrestrial magnetism. These phenomena and laws he unraveled and developed, to an extent which warrants us in regarding him as the principal source of our knowledge of their more complicated action. To our Providence meeting, in 1855, he communicated a remarkable paper, on the progress of an earthquake-wave in December previous, which was detected on our Pacific coast by means of the self-registering tide-gauges. This wave he traced from Japan,—where it had occasioned serious damage to the Russian frigate "Diana," in the port of Simoda, and where the highest of five successive waves was about thirty feet,—to Peel's Island, one of the Bonin group, where the first wave rose to an altitude of fifteen feet above the high-water level,—and thence to the shores of Oregon and California, where the disturbance was recorded at Astoria, San Francisco and San Diego; at which last-named station eight waves were traced upon the tide-gauge, the largest being six inches in height. From these observations he deduced the speed of six miles a minute as the rate of the wave's motion, and the mean depth of the Pacific as about 2300 fathoms on the San Francisco path, and about 2100 fathoms on the San Diego path.

Meanwhile he had conducted, in connection with General Totten and Admiral Davis, special researches of a highly important character, nominally independent of his position as Superintendent of the Coast-Survey. He thus served as Commissioner upon the harbors of New York, Boston, Charleston, Portland, and the Cape Fear River; and the reports contain investigations concerning the formation of obstructions in rivers and harbors, and the laws controlling the improvement and deterioration of channels, which have largely contributed to our knowledge of these laws.

At the Albany meeting of this Association, in 1856, he communicated among other valuable papers, a very important memoir from himself and Mr. Hilgard jointly, upon the general distribution of Terrestrial Magnetism in the United States. At Montreal, he described the chronometric longitude expedition, then recently carried out by him in person; the base-line which he had just measured upon Epping Plains in Maine; and the principal characteristics of the winds of the Western Coast of the United States. At Baltimore he presented an investigation of the personal equation in determining latitudes by Talcott's method; and a memoir on the tidal currents of New York Bay. At Springfield he gave a discussion of results deduced from the declinometer-observations made at Girard College, from fifteen to twenty years before; and a third memoir upon the Gulf-stream, showing the distribution of temperature in the waters of the Florida channel and straits. At Newport he added two papers containing theoretical results from the magnetic observations at Girard College, one upon the solar-diurnal variation and the other upon the influence of the moon upon the needle.

When the outbreak of the rebellion gave a new direction to the energies of all the Bureaus of the government, Bache was of course found at his post, equal to the occasion. When a secret commission of three was organized to devise measures and provide plans for the blockade of ports, or the capture of important points on the sea-board of the insurrectionary territory, the military and naval skill of officers high in command in their respective services was reinforced by the knowledge and sagacity of Bache, whose personal ability gave added value to the charts of the Survey. More than one General-in-chief looked to him for daily counsel during those days of doubt and anxiety. He carried on experiments for improving the system of signals, he arranged and carried out a multitude of military surveys, and contributed from the Coast-Survey office a large proportion of all the campaign maps furnished to commanding officers. As Vice President of the Sanitary Commission from the first, his leisure from official duties was given to patriotic and humane efforts in behalf of our soldiers. And finally, in 1863, when his native city was threatened with invasion, he offered his personal services for arranging and superintending a system of fortifications. To his intense labor here, under many difficulties and amid numerous other cares, is to be attributed the fatal malady which interrupted his work, and took him from us in the very height of his usefulness, his power and his renown. In the language of the Faculty of his own University,

> "He is justly to be regarded as a martyr to the cause of good government and the principles of human liberty, his death being directly caused by the overtasking of his faculties, in his active and never ceasing endeavors to sustain the authorities of his country against the rebellion, and to promote the efficiency and comfort of those who were fighting in our behalf."

In the spring of 1864 came the blow. It came too in the form which he had himself dreaded; as it had come to Walker before him, as it came to Faraday with him, and as the activity of nervous and mental energy in our own country especially invites it to so many of our intellectual workers. The sword wore out the sheath; the restlessly toiling brain gave way, and refused longer to obey the untiring mind. The repose, then imperative, came too late. After a summer of quiet in his camp, where he nominally continued his personal share in the triangulation which connected the New England stations with Hassler's stations in New York, he sailed for Europe in the autumn, hoping for a benefit from change of scene. A multitude of friends assembled to join in the farewell. The steamer was under the command of Captain Anderson, himself a scientist of no mean attainments, whose subsequent efficient services in connection with the oceanic telegraph, have deservedly given him a wide reputation, and whose sympathetic kindness alleviated the discomforts of the sea voyage. With touching earnestness, Mr. Bache insisted on carrying with him a set of instruments for determining magnetic constants at points to be visited during his journey. In England, France and Germany he revisited some of his scientific friends of old, but the excitement proved more than he could well bear, and this pleasure it subsequently became necessary for him to forego. It was a source of special gratification to him to meet Struve at Rome, where he was temporarily sojourning. After an absence of eighteen months in Europe he returned, and even then his friends could not utterly surrender their hopefulness. But the fatal-blow had fallen, and time only rendered its effects more manifest. At Newport, Rhode Island, on the 17th of February, 1867, his spirit cast aside the broken instrument which it could no longer govern, and returned free and rejoicing, unto God who gave it.

I will not undertake to enumerate the manifestations of honor, which were paid the lifeless clay, as it was carried from Newport to its last resting place in Washington. In New York and Philadelphia civic honors awaited it, and it reposed in state while the tokens of respect and gratitude were paid. Amid emblems of mourning, flags waving down the masts and minute guns breaking the silence, it passed along the streets. A solemn and reverent company awaited it at the national capital, and guarded it till it should be no more seen on earth. And when the last rite was over, there was not one of that great assembly who did not feel that he had lost a treasured friend, and that his country's loss was beyond utterance.

Gentlemen, I have far exceeded the limits of time appropriate for an oral address. Yet I have failed to convey more than an outline of the work he did, the services he rendered, the man he was. But my imperfect words are not his eulogy. Far and wide over this great land are scattered memorials of the love and honor of his countrymen. Above the Pacific coast a lofty mountain bears his name, and towers over the shores to which so many of his thoughts were devoted, a fitting monument to his memory. Amid the polar regions of eternal ice and snow, still his name records the services he rendered to science there; and far in the North-west, the clear expanse of a broad lake repeats the familiar sound.

But I will attempt to recall to you no more.

> "Peace! His triumphs will be sung
> By some yet unmoulded tongue
> Far off in summers that we shall not see. . . . .
> Ours the pain, be his the gain! . . . .
> Yet lifted high in heart and hope are we,
> When we remember that for one so true
> There must be other, nobler work to do.
>
> ---
>
> He is gone who seemed so great!
> Gone; but nothing can bereave him
> Of the force he made his own
> Being here; and we believe him
> Something far advanced in state,
> And that he wears a truer crown
> Than any wreath that man can weave him."

While greatness, conjoined with goodness, is held in reverence by men; while the intellectual advancement of a nation is rewarded by the gratitude of her sons; while the history of American science is read or written;—yes, until winds and tides no longer rise and fall; until shore and sea no longer follow their varied course in beach and cliff, headland and bay; until the sailor no longer fears the sunken reef, the drifting current, or the treacherous rock; until the mystic forces of earth fail to exhibit their varying play, and the lamps of heaven no longer guide the wanderer,—shall reverence, honor and gratitude adorn the name of ALEXANDER DALLAS BACHE.

CORRIGENDUM.—On page 2, line 5 from below, for "only child," read "only surviving child;" since the son, Francis Folger, lived to be five years old.

PUBLISHED WORKS

OF

ALEXANDER DALLAS BACHE.

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1863—Oct. Records and Results of a Magnetic Survey of Pennsylvania and parts of adjacent States in 1840 and 1841, with some additional Records and Results of 1834, 1835, 1843 and 1862, and a map. pp. 88. *Smiths. Contrib. to Knowl.*, vol. xiii, art. viii.

1864—May. Discussion of the Magnetic and Meteorological observations made at the Girard College Observatory, Philadelphia, in 1840 to 1845. Third section, comprising Parts VII, VIII, and IX. Vertical Force. Investigation of the Eleven (or Ten) Year Period, and of the Disturbances of the Vertical Component of the Magnetic Force, and Appendix on the Magnetic effect of the Aurora Borealis; with an Investigation of the Solar Diurnal Variation, and of the Annual Inequality of the Vertical Force, and of the Lunar effect on the Vertical Force, the Inclination and Total Force. pp. 72. *Smiths. Contrib. to Knowl.*, vol. xiv, art. ii; *C. S. Rep.*, 1863, p. 156.

1865—Jan. Discussion of the Magnetic and Meteorological observations made at the Girard College Observatory, Philadelphia, in 1840 to 1845. Fourth section, comprising Parts X, XI, and XII. Dip and Total Force. Analysis of the Disturbances of the Dip and Total Force; Discussion of the Solar Diurnal Variation and Annual Inequality of the Dip and Total Force; and Discussion of the absolute Dip, with the Final Values for Declination, Dip and Force between 1841 and 1845. pp. 44. *Smiths. Contrib. to Knowl.*, vol. xiv, art. iii; *C. S. Rep.*, 1864, p. 183.

1844 to 1863. Annual Reports of the Progress of the United States Coast-Survey.

1844 to 1848. Annual Reports of Superintendent of Weights and Measures.

1855 to 1863. Tide Tables for the use of navigators, prepared from the Coast-Survey observations, annually.

Also, the following Reports concerning Harbors, jointly with Messrs. Totten and Davis:

1853—March. Report upon Cape Fear River and Harbor.

1854—Oct. Report of Portland Harbor Commission.

1855—March. Second Report of the Commissioners on Portland Harbor.

1855—Dec. Report of Advisory Council of the New York Harbor Commission. *New York Assembly Doc.*, 1856, No. 8.

1856—July. Report of the Advisory Council of the New York Harbor Commission, recommending certain lines in the East and North Rivers, and in Brooklyn. *New York Senate Doc.*, 1857, No. 40, p. 107.

1856—July. Report of the Advisory Council of the New York Harbor Commission on Gowanus Bay and its Improvements. *New York Senate Doc.*, 1857, No. 40, p. 118.

1856—Sept. Report of the Advisory Council of the New York Harbor Commission, on the Hellgate Passage, regarded as a channel of approach to New York Harbor. *New York Senate Doc.*, 1857, No. 40, p. 137.

1856—Oct. Second General Report of the Advisory Council to the Harbor Commissioners, on Lines in New York Harbor. *New York Senate Doc.*, 1857, No. 40, p. 81.

1856—Dec. Report of the Advisory Council to the Commissioners on Harbor Encroachments of New York, in regard to safe and commodious Anchorages in New York Harbor, for the Purposes of the Quarantine of Vessels. *New York Senate Doc.*, 1857, No. 40, p. 132.

1856—Dec. Report of the Advisory Council to the New York Harbor Commissioners, upon Harlem River and Spuyten Duyvil Creek. *New York Senate Doc.*, 1857, No. 40, p. 151.

1857—Feb. Report of the Advisory Council, of Lines for the East River, for the Shore of Long Island, and the East Shore of Staten Island. *New York Senate Doc.*, 1857, No. 126, p. 9.

1857—March. Report of the Advisory Council to the New York Harbor Commissioners, upon the Comparative Map of New York Bay and Harbor, and its approaches, prepared by the Coast-Survey in March, 1857. *New York Senate Doc.*, 1857, No. 126, p. 12.

1860—March. Preliminary Reports of Commissioners on Boston Harbor. *Boston City Doc.*, 1860, No. 37.

1860—Dec. Second Report of United States Commissioners on the Condition of Boston Harbor. *Boston City Doc.*, 1860, No. 97.

1860—Dec. Special Report of the United States Commissioners on Boston Harbor, on the Relation of Mystic Pond and River to Boston Harbor. *Boston City Doc.*, 1861, No. 12.

1860—Dec. Report of the Advisory Council of the Joint Committee of the Massachusetts Legislature on the subject of a Ship Canal to connect Barnstable Bay and Buzzard's Bay. *Mass. Pub. Doc.*, 1864, No. 41.

1861—Sept. Fourth Report of the United States Commissioners on Boston Harbor. *Boston City Doc.*, 1861, No. 62.

1861—Sept. Fifth Report of the United States Commissioners on Boston Harbor. *Boston City Doc.*, 1861, No. 63.

1863—April. Sixth Report of the United States Commissioners on Boston Harbor. *Boston City Doc.*, 1863, No. 53.

1864—March. Seventh Report of the United States Commissioners on Boston Harbor. *Boston City Doc.*, 1864, No. 33.

1864—March. Eighth Report of the United States Commissioners on Boston Harbor. *Boston City Doc.*, 1864, No. 34.

ADDRESS

OF

FREDERICK A. P. BARNARD, LL. D.

EX-PRESIDENT OF THE ASSOCIATION.

Gentlemen of the American Association for the Advancement of Science:—

You are assembled to day for the third time since the revival of your association in 1866. On the occasion of that revival it was my privilege to be your presiding officer—a privilege and an honor which I shall always remember with the highest gratification and pride—but of which one of the resulting obligations has never been fulfilled. My unavoidable absence at your last meeting had, as I supposed, been tacitly accepted as a sufficient apology for the failure; but your standing committee, with a fidelity of recollection which I had hardly anticipated, have preserved the record of the debt, and have consequently assigned me the not unpleasing duty of welcoming your return at this time to your annual deliberations.

The city which, on the present occasion, has extended to American science the encouragement of its generous hospitality, is one which has been heard of before in connection with large assemblies professing to represent our country in one or another of its aspects. But this is the first time that a great convention has been assembled here which could claim to be called in the strictest sense national; a convention having no aims likely to engender suspicion in any quarter, and intent on no proceedings liable to be watched by any jealous eyes. For this is the first of the national conventions which the political, social, commercial and intellectual prominence of Chicago has attracted to this spot, of which the declared platform has been entirely catholic and universally acceptable; the first which could justly hope to enlist the sympathies of all parties, all creeds, all states, all sections alike. It is gratifying to know that there is one subject in regard to which the whole world of mankind have a common interest, one subject on which there can be but one party. Such a subject is that which occupies us. For the object of science is truth. Its progress is the progress of civilization, its encouragement is the encouragement of the arts of life, and the enlargement of the comfort and the happiness of the human race.

Of these truths your presence here to day is a striking illustration. The very possibility of such a general gathering of the votaries of science, is a possibility which science herself has created. You are assembled from every point within the limits of our vast territory. Some of you have left your homes a thousand, some fifteen hundred, miles behind you. Three days ago you were in your laboratories, your lecture halls, your quiet studies, pursuing tranquilly your ordinary occupations, and to day finds you assembled here. The power by which you have been enabled thus to triumph over space is the gift of science to the industrial arts. The iron way which has brought you is her creation; the iron arm which has dragged you, has been clothed by her with its herculean strength. Were engineering science at this time no farther advanced than it was when the great commonwealth of Illinois, but a brief half century ago, first took her place in the sisterhood of the states, the time it would consume, the expense it would involve, and the excessive fatigue it would entail to bring together a body of representative men from points so widely separated as the banks of the St. Lawrence, and the shores of the Gulf of Mexico, would render an assembly like the present a thing wholly impracticable. It is then to science herself that we owe it that we are able, with so little inconvenience, to hold our annual consultations for the advancement of science.

Yet it may be doubted whether this fact has to any great degree occupied your thoughts on your way hither. You have been much less intent upon what science has done than upon what she has yet to do. Accomplished facts have for the investigator but a secondary interest. He is even prone almost to forget them in the earnestness with which he addresses himself to the things which remain to be accomplished. There are a thousand truths, and applications of truths, which in the day of their novelty fastened universal attention, but which having become familiar, and having been added to the permanent treasury of knowledge, have at length almost ceased to be remembered as the gifts of science to civilization. Who, for instance, that uses now the electric telegraph, thinks of the slow process of evolution by which, in the hands of Œrsted and Ampère and Arago and Henry, the laws of electro-dynamics were gradually unfolded; or of the happy sagacity by which, in the laboratories of Daniell and Grove and Bunsen, the powerful and persistent electro-motors now in universal use were conceived and perfected; or recognizes the fact that but for the combined results of all these labors, this wonderful instrument of communication could have had no existence? Or who that pauses for a moment before the show window of a print shop in any one of our great cities, to inspect curiously the latest photograph of the pyramids of Egypt, the mountain gorges of California, the glaciers of the polar seas, or the ruins of Mexico or Peru, remembers, if he ever knew, the weary and disappointing toils protracted through years, by which Daguerre and Talbot, and Niepce de St. Victor succeeded at last in permanently fixing the fleeting images of the camera?

These illustrations lead me to remark that, in the history of every new truth brought to light by scientific investigation, there are three stages corresponding to three periods in the life of man. The first may be likened to the period of birth and infancy, deeply interesting to the parent or to those for whom infancy has charms, but regarded with indifference by the world in general; the second answers to the youth of the individual, during which feebleness is ripened into strength, and the helpless child is trained to a useful vocation; and the third is the period of maturity in which the full-grown man becomes merged in the society which surrounds him, where, though his usefulness is recognized, few trouble themselves to inquire beneath what roof he was cradled, or under what tutelage his nascent powers were developed.

In the observation of Volta upon certain curious disturbances of electrical equilibrium, we have an example of a truth in its infancy: in electro-chemistry and electro-magnetism, in the alkaline metals, in the galvanoplastic arts, the electric telegraph, and the light houses of Dungenness and La Hève, we have the same truth in its vigorous maturity. In the mercurial vacuum produced first by Torricelli, we have another elementary truth. In the engine of Newcomen, destined by successive transformations to pass into the powerful motor which is the soul of modern industry on the land and of modern commerce on the ocean, we have this also in its later phases of development and useful application. Galileo demonstrated the isochronism of the oscillation of suspended bodies; the consequence is before our eyes every day in the pendulum clock. Wollaston marked the curious fact of the transverse striation of the solar spectrum. Bunsen and Kirchhoff converted this into an instrument of chemical research, and Huggins, Miller, Secchi and others have more recently read by its help the constitution of the sun, the fixed stars and the nebulæ. The applications of truths of science to the industrial arts are so numerous, that there is scarcely any article we handle in our daily life, which does not furnish an illustration. And all these things serve to show how matured science passes gradually into common knowledge, while nascent science is always the exclusive possession of the limited class who make its culture a specialty.

This class, though limited, hold to the world in general a position and a relation exceedingly important. They may be likened to the pioneer corps of an army marching through an unexplored and difficult country. They break the path in which the multitude are to follow. Their place is in the front, and their faces are turned toward the wilderness which the invasion has not yet reached. And so our pioneers are always found energetically operating in the border region which divides the known from the unknown; and they too like the others, forgetting or disregarding the things that are behind, are chiefly intent on effecting new encroachments upon territory as yet unconquered. Their task is difficult. Their numbers are few and scattered. They feel, therefore, the need of often drawing near together, and taking counsel of each other, for the promotion of their common interests. It is thus that they have been led to organize themselves into permanent associations for mutual assistance and encouragement. Such associations have accordingly grown up in every land in which science is cultivated, and their influence has been eminently beneficial in promoting unity and efficiency of action, and in keeping alive the zeal of those who compose them, for the advancement of the common cause. On this side of the Atlantic your association is the most prominent example of this class of organizations.

It is a usage sanctioned by time, that your retiring president, in laying down his office, should improve the opportunity to glance briefly over the general field of science, and present in outline the principal facts of progress which have marked the history of the year that has passed. It is not quite easy, nevertheless, in an area of intellectual activity so extended, and where the laborers are so numerous, to make such a survey exhaustive; and where investigation is so steadily continuous, it is equally difficult to decide how much belongs to the history of a particular year, and to adjust the achievements of science severely to the calendar.

This last difficulty is illustrated in the very first topic to which I shall draw your attention. Since your last meeting the Smithsonian Institution has published the fifteenth volume of its "Contributions to Knowledge," containing the new investigation of the orbit of the planet Neptune, by Professor Simon Newcomb of the United States Naval Observatory. But this able discussion, though only now formally published, already dates back some two or three years, and has been for some time in the hands of astronomers. It has, nevertheless, an interest which will justify a brief mention here. The theory of this planet has long been in a condition so unsatisfactory as to demand thorough revision. And since our country was the first to produce an investigation of the perturbations of the body, and an ephemeris of its movements, in the contributions of Professor Peirce to the Monthly Notices of the Royal Astronomical Society, and in the discussions and tables published by the lamented Walker among the earlier Smithsonian papers, it is gratifying to find that the work of reconstruction has been undertaken and has been so well performed by an American hand. The necessity of the revision appears in the fact that, in 1863, the ephemeris of Walker was 33″ in error, and that of Kowalski, published some years later, 22″. Before the end of the present century, the error of either of them must become enormous—not less than 5′. The earlier theories were founded on a very limited period of observation; that of Walker on the observations of but little more than a year, during which the planet had moved only 2½°. In the twenty years which have since elapsed, the progress of this body in longitude has exceeded 40°; so that the influence of errors of observation upon the value of the elements is tenfold less than it was when Mr. Walker undertook the investigation. Professor Newcomb has produced a theory which represents the observations since the discovery of the planet in 1846, and also the unconscious observation of the same body by Lalande in 1795, with a maximum error, usually negative, less than 2″ and a mean error less than 1″. He has discussed the question whether the observations can be better satisfied by any changes in the elements of the orbit and in the masses of the disturbing planets, and has concluded that, while no admissible change in the mass of either Jupiter or Saturn will effect the perturbations of Neptune, neither will any correction of that of Uranus do so which is not at once excessive and unreliable. Considering the question whether the remaining errors are owing to an extra-Neptunian planet as yet unknown, he decides against the present evidence of the action of any such body; but regards this negative result as not by any means decisive against the existence of such a planet, because the errors of a slow moving planet require for their developement a much longer time than those of planets of short period, these errors developing themselves not in proportion to the time, but to the square of the arc moved over. When Neptune was detected through the perturbations of Uranus, this latter planet had been observed through an arc of 270°; and the two planets had been in conjunction. Yet notwithstanding these favoring circumstances, Uranus had only departed about 5″ from an elliptic orbit since its first discovery. Had it been observed only through an arc of 120° (something less than half as great) its deviations would not have exceeded 1″. Whether, therefore, Neptune is the exterior planet of the solar system or not, cannot be at present positively decided, and no evidence to throw light upon the question can be looked for in the anomalies of its movements during the present century.

This valuable contribution to astronomical science suggests another, more recent, by the same hand. From the observations of the planet Mars in opposition made in 1862, at Washington, Albany, Greenwich, Leyden, Helsingfors and Pulkova, in the northern hemisphere, and at the cape of Good Hope and Santiago de Chili in the southern, Mr. Newcomb has deduced a new value for the solar parallax, which he makes to amount to 8″.855, with a probable error of 0″.02. The value of this element is perhaps the most important of all constants in physical astronomy; since on it depends our knowledge of the distance of the sun, which is the unit of measurement in the celestial spaces. The value heretofore received, 8″.58, deduced by Encke, from the observations of the transit of Venus made in 1769, has always been regarded with a certain distrust, founded on circumstances not wholly satisfactory attending the Norwegian observations. Mr. Leverrier had greatly strengthened these suspicions, by discussing the observations of the sun, in reference to the lunar inequality of its longitude, which led him to the conclusion that the parallax should be increased to 8″.95. He had arrived at a similar result from a careful revision of the theories of the planets Venus and Mars, according to which it appeared that such an increase of the parallax was necessary to satisfy the observations of those bodies.

In the mean time Mr. Fizeau attempted his ingenious determination of the velocity of light, by observations made at limited distances on the earth's surface. Mr. Foucault subsequently engaged in the same experimental investigation, with apparatus constructed upon an original principle. It is easily seen that if we have the true velocity of light, we have indirectly and at once the solar parallax. For the aberration of the fixed stars gives us a determinate relation between the velocity of light and that of the earth in its orbit. And the velocity of the earth with its periodical time, furnishes directly its mean distance from the sun. The results obtained by both these experiments, inclined toward the smaller value. Mr. Foucault more recently resumed his experiments; and having taken extreme precautions to exclude the possibility of sensible error, he arrived at new results which led him to announce the parallax at length, at 8″.86. It is interesting to observe the near approach to identity between this result, and that reached by Professor Newcomb by a method purely astronomical. Professor Newcomb combines this determination with several others, including that deduced from the experiments of Foucault just mentioned, and those which have been furnished by the micrometrical measurements of Mars by Professor Hall made in 1862, the parallactic equation of the moon, the lunar equation of the earth, and finally the transit of Venus of 1769 recomputed by Professor Powalky; the conclusion arrived at, after assigning its proper weight to each determination, being, that the true parallax is 8″.848, or 8″.85, with a probable error of 0″.013. This would make the mean distance of the sun 92¼ millions of miles, or about 3 000 000 less than the value heretofore received.

A very curious question has been agitating the astronomical world for more than a year past, in regard to the asserted disappearance of the lunar crater Linnæus. This crater was described by Mädler as very large, being not less than six miles across, and also as very deep. In the latter part of 1866, it was announced by Dr. Schmidt of Athens, that it had entirely disappeared and had been replaced by what he described as a whitish spot. Subsequently within this spot and at its centre, it was further asserted that there had appeared a minute dark point, seen independently by Schmidt at Athens, Secchi at Rome, and Lyman at New Haven, indicating the formation of a new crater; and this has apparently grown larger and gradually changed its place. If these observations, which seem as yet to be received by many with reserve, should be confirmed, quite a new interest will be given to the study of the physical aspects of our satellite.

On the 21st of August, 1867, Jupiter was seen under those rarely occurring conditions in which he appears without satellites. Two of the satellites were in eclipse, and two were projected on his disk. At this time the appearances presented by the satellites in transit were so remarkable as to attract general attention. The light of the fourth satellite was extremely feeble so that it was described as appearing no brighter than its shadow on the disk. There was noticed, also, by one observer, a singular configuration of light and shade occupying one half the disk of the third satellite, such as had, on a former occasion, been seen on the other half. This suggests the possibility that the commonly received opinion, viz., that all the satellites of the planets like the moon, turn constantly the same face toward their primaries, is not true. The dimness of the fourth satellite in transit, and the varying brightness of all of them in similar circumstances, were noticed a century and a half ago by Pound; and the latter phenomena were at the time ascribed by him, very plausibly, to the proper rotation of those bodies on their axes and the unequal reflecting power of different parts of their surfaces. The identity of the rotation and revolution period of the secondaries must evidently no longer be taken for granted.

Of the family of small planets between Mars and Jupiter, the number rapidly increases. Since your last meeting, eight additional members have been announced, of which three are due to an American observer, Professor Watson of Ann Arbor. The total number now known, so far as I am at present informed, for it is not quite safe to be positive, is exactly one hundred. The earliest body of this class ever discovered, and except one probably the largest, was first seen by Piazzi on the very first day of the present century. It is interesting to remember, at a time when every few months adds a new member to this already very numerous group, that this first of the minor planets was lost soon after its discovery, and was not recovered again for nearly eleven months. Piazzi's last observation, previous to the loss, was made on the 11th of February, 1801, when illness interrupted his labors. The rediscovery was made by De Zach with the aid of an ephemeris computed by Gauss, who had never seen the object, on the 31st of December, of the same year. Of seventy one of these small planets Mr. Stone has formed a table of probable dimensions. He places Vesta, discovered fourth in order in 1807, and esteemed at the time of its discovery the smallest of the four, at the head of the list, and fixes its diameter at 214 miles. Echo stands lowest in his catalogue, its diameter being estimated at only seventeen miles. It illustrates the increasing frequency of discovery in this class of celestial objects in recent years, and at the same time the high degree of perfection of the modern star maps which has made this frequency possible, that after the discovery of Vesta nearly forty years elapsed before another addition was made to the list, and that the first half of this century yielded in all, only ten. Since 1850, the average rate of discovery has been about three a year.

The period of the diurnal rotation of Mars has recently been redetermined by Mr. Proctor of the Royal Astronomical Society from a comparison of Hooke's observation made March 12, 1666, with recent observations in 1856, 1864 and 1867, the longest interval embracing 71 538 rotations. The result shows the necessity of a correction amounting to about a second, of the period assigned by Mädler from the results of seven years observations. It exceeds by about a tenth, or fifteen hundredths of a second, the determination made by Kaiser.

Our countryman, Mr. Cleveland Abbe, formerly of the Pulkova Observatory, has made a careful and laborious calculation of the parallax of Sirius, from the observations of this star with the transit circle made at the Cape of Good Hope. He concluded the parallax to be not less than 0″.17 nor more than 0″.37 making the distance to fall between six and thirteen hundred thousand times the diameter of the earth's orbit. The determination by Henderson, deduced from observations at the same point, lay between these limits and near the lower, implying a distance of 900 000 similar diameters. When it is considered that this star is by far the brightest in the heavens, exceeding, if we adopt the scale of Secchi, perhaps forty times the brilliancy of 61 Cygni, whose parallax is near the maximum given by Abbe in this determination, we may form some feeble conception of what must be the intrinsic splendor of this magnificent globe.

Mr. Abbe has also contributed to the Royal Astronomical Society an interesting examination of the distribution of the nebulæ. The clusters and planetary nebulæ he considers to belong to the milky way, but other nebulæ form independent systems.

On the seventeenth of August in the present year, a day now near at hand, there will occur a total eclipse of the sun, under circumstances remarkably favorable to the duration of the obscuration. It is stated by Dr. Weise, before the Royal Astronomical Society, that there are but two total eclipses in history which, in respect to duration, can be compared to this. The first is the famous eclipse said to have been predicted by Thales, which happened in May, 583 B. C., during a battle between the Lydians and Medes, filling both armies with consternation; and the second was that which occurred in June, 1435, in Scotland, where it was long remembered as "the black hour." The eclipse now just at hand occurs near the perigee and almost at the moon's minimum distance from the earth. It happens also precisely in the moon's ascending node. Where the centrality takes place on the meridian, the two bodies will be nearly in the zenith, so that the augmentation of the diameter in altitude will be a maximum, and the rate of motion of the shadow on the earth's surface a minimum. Thus the total obscuration will be hardly less than seven minutes. It is unfortunate that this interesting phenomenon will occur in a portion of the earth least favorable to its observation. It commences in Abyssinia, crosses southern Arabia and Hindostan, the Gulf of Siam where the duration of the total phase is maximum, the islands of Borneo, Celebes, and the southern part of New Guinea to the New Hebrides, where it ends at sunset. The extraordinary opportunity which this eclipse offers for studying the phenomena of the solar atmosphere, and the nature of those extraordinary rosy flames, so called, occurring during totality, which have for thirty years past attracted so much attention and excited so deep an interest on the part of astronomers, will not, however, remain unimproved; very large and thorough preparations having been made by the astronomers of England and continental Europe, to secure the most complete record possible of all the phenomena, including photographic views of the phases from many different stations. The results obtained by such means during the eclipse of July 18, 1860, notwithstanding the disappointments which attended some of the distant expeditions then sent out, were of great interest; and it is to be hoped that the similar opportunity which now presents itself will be a means of securing a rich addition to the materials in possession of science for the study of solar physics.

This question indeed of solar physics, the constitution of the sun's atmosphere, his photosphere, and his central nucleus, is one which has attracted to itself recently much laborious attention. The solar spots have been especially the subject of patient and persevering study, furnishing as they do evidence of the extraordinary activity and energy of the forces which are constantly in operation producing changes in the luminous envelope. Mr. De la Rue, Mr. Balfour Stewart and Mr. Loewy, have particularly devoted themselves to the determination of the laws governing the variations of the total area of spot-surface, and its distribution over the face of the sun. This area was ascertained for successive epochs by the actual measurement of enlarged photographs of the sun. The face of the sun being divided into thirteen equal portions or lunes, each corresponding to a day's motion, by great circles passing through its poles, the total area of the spots in each lune was ascertained for several series of observations, embracing a period of many months, and the results tabulated. From a series of elaborate tables thus formed, it was presently made apparent that the region of maximum spotted area is not always similarly situated on the sun's disk as seen from the earth, but passes from left to right. The existence of an external influence affecting the phenomenon is thus made very probable, and by comparing the changes with the movements of the planets, Venus seems to be distinctly indicated as the body exercising this influence. The recurrence of the maximum spot-area in the same longitude relatively to the earth on the sun's surface, is evidently periodical, and the period corresponds to the synodical period of Venus. Also the average size of a spot seems to be maximum on the side of the sun which is turned from the planet. A similar effect is ascribed to Jupiter, but the investigators show that this, though perhaps as powerful, is not characterized by so large inequalities as that of the smaller body, whose comparative inferiority of mass is more than compensated by its proximity to the sun. The observers deduce the additional conclusion from their inquiry, that the spots are, on the whole, nearest to the solar equator when Venus is in the plane of that great circle, and farthest from the solar equator, when the heliographical latitude of the planet is greatest. This observation gives significancy to the fact long known, that the solar spots are never seen far distant from the equatorial region.

As to the cause of these spots, or more generally and more properly perhaps, the nature and determining causes of the photosphere in which they make their appearance, a conjectural theory must be sought in the known influence of exceedingly high temperatures and pressures upon the chemical condition of all bodies. This subject has been discussed from time to time with great ability, by Mr. Faye of the Imperial Observatory of France, by Mr. Balfour Stewart of London, and by Mr. Sterry Hunt of Montreal. These gentlemen conceive the sun to consist of an immense mass of matter in a gaseous condition, but with its chemical affinities held in check by heat, so that all the elements are mingled together but not combined. The condensation of this gaseous mass is such as to give it a density not less than that of water. Radiation from the surface of the mass produces a depression of temperature sufficient to permit certain elements to enter into combination; forming finely divided solids or liquids, which being held in suspension in the surrounding gases and vapors, become intensely luminous, as phosphoric acid is momentarily luminous when formed by combustion in oxygen gas, or as carbon is luminous in the candle flame. Supposing the general surface of the sun to be thus overspread with a luminous envelope, the theory of the formation of the spots connects itself with that of the lines of more than average brightness, or the faculæ. And it seems to be established that while these latter are in the nature of immense elevations or wave crests of the luminous matter in suspension, so the former are vast depressions. Or rather, while the faculæ are produced by *ascending* currents, the maculæ are formed by currents *descending*. The descending currents carry the luminous precipitate into a region of greater heat, where it is decomposed and disappears. Above the photosphere there is evidence of the existence of an atmosphere, extending perhaps a million of miles, perhaps much more, above the visible surface, and in this, on those rare occasions furnished by total eclipses, are seen the irregular red clouds or projections which have been so much discussed of recent years, and of which we hope soon to know much more. These flames are to be examined with the spectroscope and the polariscope during the eclipse of this month, and their photographic images will be secured at many stations. Observations will also be made with a view to the detection, if possible, of the intra-mercurial planet, of which the existence has been suspected, and which was looked for in 1860 without success.

The star-shower of November 14, which had been witnessed in Europe on the 14th of November, 1866, returned, as predicted, one year later, in a more westerly longitude, and was extensively observed on this side of the Atlantic. This return has furnished fresh material to confirm the received theory of these displays. Periodical meteors may be regarded as proceeding from what are called by Schiaparelli cosmical clouds, which, from having a cometary character, have been transformed by perturbations produced by the planets into currents or streams, revolving round the sun in extremely elongated orbits, and tending constantly to become rings. Mr. Newton had pointed out that the period of revolution of the cosmical cloud producing the November meteors must be one of five, of which four were less than the earth's period, and one much larger, or thirty two and a quarter years. Soon after the return of the shower in 1866, Mr. Adams took up the investigation of the question, by computing the perturbing effect of the planets upon the movement of the node, on supposition, first of a period less than a year, and secondly, of one extending to thirty two and a quarter years. The first supposition gave less than half the observed amount of movement, and the second accorded so closely with observation as to leave no doubt of its truth. Mr. Leverrier regards this meteoric stream as having originated from the perturbation of a cosmical cloud or comet produced by Uranus about seventeen centuries ago.

A very curious fact connected with this subject, is the recent discovery that certain bodies which have been distinctly recognized as comets, are unquestionably members of periodical meteoric groups. It was found by Schiaparelli, that the great comet of 1862 is only one of the August meteoroids, and that the first comet of 1866 is a member of the November group, near the head of the train. Another curious fact, discovered by Hoek of Utrecht, is that there are double or triple systems of comets, or systems in which two or three must have had a common origin, though their perihelion passages have been separated in time by years.

Some observations on the spectra of the meteors of 1866 were made by Mr. Browning of London. Those of different bodies presented appearances remarkably different. They were generally continuous, some presenting all the usual colors except the violet, which was perhaps not seen on account of its feebleness. In some, the yellow was greatly predominant, though the other colors were present. Some were purely yellow, or nearly so, and a few purely green. This mode of observation, if pursued, may lead to useful results.

The spectroscopic observations of the sun, stars and nebulæ, continue to be prosecuted, those of the stars especially by Father Secchi. He has arranged the stars, as characterized by their spectra, in three groups, the first embracing only such white stars as have a black band in the part of the spectrum between the green and blue, and another in the violet; the second distinguished by possessing colored bands in the red and orange; and the third giving only fine lines. This last embraces Arcturus, Capella, and our own sun. To this, also, the red stars generally belong. Few stars are so exceptional as not to be referable to one or another of these classes, but one, at least, has been found by Secchi, of which the spectrum resembles that of the sun observed through an absorbing solution. This indicates an atmosphere of peculiar character and of extraordinary density.

To the metals discovered spectroscopically to exist in the sun, Mr. Angstrom adds manganese. This makes the twelfth, or if we call hydrogen a metal, the thirteenth, certainly ascertained to exist in our central luminary; and the presence there of copper, is also probable. None of the precious metals are yet among the number.

Mr. Stockwell has recently published an extension backward of his chart of the eccentricity of the earth's orbit, for a second million of years. Should the theory discussed two years ago at Buffalo, which connects the glacial periods of geology with this secular inequality, be found tenable, the chart of Mr. Stockwell may serve a useful purpose in regulating geological chronology. An interesting inference deduced from the inspection of the chart, is that the periods of corresponding maximum and minimum eccentricity occur at intervals of 1450000 years. Another is, that two large maxima are sometimes immediately consecutive, and sometimes separated by a number of minor maxima, so that, on supposition that such a maximum brings a glacial period, the interval between two such periods, or two severe periods of such a character, may be very variable.

It seems due to the memory of one of the most deserving and most highly esteemed of American astronomers, now passed away, that I should mention in connection with these astronomical notices, the recent publication of the fifth volume of the Annals of the Harvard Observatory, containing the extremely interesting observations of the great nebula in Orion, made by Professor George P. Bond. This work completed and edited by Professor Safford, now of the Dearborn Observatory of Chicago, constitutes one of the most elaborate and valuable studies of a nebula that has ever been made; and it will be a standard work of reference in all future observations of the changes in form or brilliancy of this most beautiful object of its class, and of the stars associated with it.

In geology, while the contributions to particular knowledge have been numerous, and evidences of activity in every quarter are satisfactory, there is little which is new in regard to theory. Mr. Lombardini has communicated to the Royal Institute of Lombardy, some evidences of glacial action in the great depression of central Africa, and supposes that the mountains of Abyssinia and more southern regions will furnish confirmatory evidence to the same effect. He concludes, as Agassiz has done from his observations in Brazil, that during the glacial period the ice-envelope overspread the entire planet. The tenability of this theory is now one of the questions of highest interest yet unsettled. There is some ingenuity in the suggestion recently made, that glacial phenomena in low latitudes as well as the evidences of a former tropical climate in the polar regions may be accounted for by supposing the upheaval of mountains in extra-tropical regions to have caused a gradual sliding of the earth's external crust upon the central fluid, in consequence of the centrifugal force of the protruded masses, tending to drag them down to the equator. It is certain that the amount of flexure which the crust would on this supposition have to undergo, would not be sufficient to produce rupture, and the suggestion is free from the mechanical difficulties which attend the hypothesis of a change of position of the earth's axis of rotation.

The geological researches of Mr. Raphael Pumpelly in Japan, Mongolia, and China, have at length appeared among the Smithsonian Contributions. They furnish a fund of valuable information in regard to an extensive region of the earth which has been little explored. Dr. Newberry's discovery of the mesozoic character of the great Chinese coal deposits, from fossil plants gathered by Mr. Pumpelly, is one of the most interesting facts brought to light by modern geological research.

Archæological inquiries, especially researches concerning the physical and social condition of prehistoric man, and the chronology of the several phases of his existence, continue to be pursued with unabated zeal. New traces of the primitive races are continually appearing, some of them in unexpected quarters, as for instance remains of pile-dwellings in the Thames at London, and flint implements associated with the bones of recent and extinct mammals at Paris. The most important novelty in the way of discovery in this department, however, is that of a human skull, in the lower post-pliocene deposits of the plain of the Aretino in Italy. This occurs in a regular stratification consisting of lacustrine clays of great thickness, embracing bones of *Elephas primigenius*, *Bison priscus*, *Cervus euryceros*, and others. Stone implements were found associated with the skull and with these bones. Whatever may be said of the date of deposits like these, which must be very ancient, there can hardly be a doubt that the computations which have been founded on the observed progress of the formation of alluviums in the neighborhood of former discoveries, have led to extravagant estimates of their antiquity. An interesting reëxamination of the phenomena presented in the valley of the Somme, has recently been published by Professor Andrews of Chicago.

Chemistry is so fertile a subject that it would be hopeless to undertake anything more than to notice, here and there, a point of interest in the history of its progress. Two improvements in technical chemistry are announced, which may, possibly, be found to be of great commercial, as well as of scientific importance. One of these is a method which has been protected by patent in France, for manufacturing sulphuric acid without the use of large leaden chambers. There is nothing new about the process in principle, but much in the details of application; the apparatus being forty times reduced in bulk, and the acid produced almost entirely free from impurities. Considering the importance of this great mineral acid—the most important probably, at once to chemistry and the arts of all chemical reagents—anything which tends to simplify the process or diminish the cost of its production, or to improve the quality of the product, must be esteemed a very essential benefit in which science and the industrial world share equally.

The other novelty referred to, is a substitute for the famous process of Leblanc, for the preparation of carbonate of soda from the chloride. In this the sulphuric acid is replaced by sesquioxide of chromium, heat being applied and a current of steam directed upon the mass. A second heating with charcoal, which converts the chromate into carbonate and revives the sesquioxide, completes the operation. The process is ingenious and beautiful. Whether it will prove commercially a success remains to be seen.

Contributions to organic chemistry have been too numerous to admit of a detailed review. In this department, Berthelot continues to stand preëminent. His labors on the hydrocarbons have been productive of many extremely interesting results, comprising among them the synthesis of several organic compounds. A year ago, he effected the direct synthesis of acetylene by the union of carbon and hydrogen; and more recently, from acetylene, he has succeeded in producing oxalic acid. Another very interesting result obtained by the same investigator, consists in the discovery of a universal method of transforming any organic body whatever into a hydrocarbon, with the amount of carbon unchanged, and the hydrogen a maximum. The reagent which produces this effect is hydriodic acid.

The method of Berthelot just mentioned for producing oxalic acid synthetically, is equalled if not surpassed in interest by a discovery of Dr. Drechsel, by which the same acid is produced by a reduction of the carbonic. This result, on its announcement in the chemical society of London, was received with expressions of high and just admiration.

Considerable interest has also been excited by the researches of Mr. Griess, upon a series of organic compounds in which the hydrogen is replaced by nitrogen, which have led him to the discovery of a class of organic explosives, exceedingly by energetic. It is a curious fact that every known explosive, liquid or solid, is a compound into which nitrogen enters.

In physico-chemistry, a very ingenious theory has been proposed by Mr. M. Carey Lea, of Philadelphia, to explain the action of light upon photographic salts, especially the iodide of silver. The prevailing opinion has been, that the salt undergoes a partial reduction, and thus acquires the property of combining with the vapor of mercury. The improbability of this hypothesis is shown by the fact long known to photographers, that the plate loses this power of combination, and recovers its sensitiveness to light, if kept for a time in a dark place. Mr. Lea is of opinion that the change is physical and not chemical, and that the molecules acted upon by the light have their vibratory motions—their *vis viva*—exalted, by sympathy with the luminous vibrations; as sonorous bodies are excited by a sound in harmony with that which they are capable of yielding. He proposes to give to this influence the name actinescence. This theory serves to explain the remarkable phenomenon discovered by Niepce de St. Victor, viz:

that light may be "stored up" or absorbed by bodies which have been exposed to it, so that afterwards such bodies will act chemically upon sensitive plates in the dark; an effect which continues for a considerable time. The plausibility of the theory will doubtless secure for it a general reception.

Dr. Emerson Reynolds of Dublin, has recently shown that photographic plates which have been impressed by light, may be quickly restored to sensitiveness, and the latent images obliterated, by exposure to ozone. He regards this fact as a conclusive proof that the image is produced by a chemical change, and that what he calls the mechanical theory, which is the theory of Mr. Lea, is untenable. The premises do not seem to justify the conclusion. The molecules of ozone of which the density has been recently found by Regnault to exceed 1″.5, may very probably possess precisely such a vibratory period as to interfere with the vibrations of the molecules of the iodide, though the oxygen and nitrogen of the air have not such an effect.

It has been demonstrated by Professor Knoblauch that heat, like light, transmitted through doubly refracting crystalline films, suffers interferences; so that in the polariscope, if the analyzing prism is rotated, the calorific rays transmitted in opposite azimuths are complementary to each other. This furnishes a new and very interesting evidence of the fact that light and heat are physically identical, and that their differences are only differences relatively to our perceptions.

To the apparatus of static electricity a valuable contribution was made a year or two since, in the now well known induction machine of Mr. Holtz. A similar service was rendered to dynamic electricity in the ingenious magneto-electrical machine contrived by Mr. Wilde. At the Universal Exposition in Paris during the last year, Mr. Ladd, of London, exhibited a mechanical electro-motor of still more simple construction, in which the currents generated in the wire wrapping of a revolving armature are employed to excite the electro-magnet by which the armature is itself excited; while a second revolving armature is employed for the purpose of obtaining an independent current to be used in electrolysis or for the production of light.

This machine of which the effects are surprisingly powerful has excited an extraordinary interest for the promise it holds out of practical usefulness, and for the illustration it affords of the direct conversion of dynamic into electrical energy.

Two new chemical electro-motors have been announced, one by Mr. J. B. Balsamo formed of plates of iron for both positive and negative elements, immersed in cells with a porous partition, filled on one side with dilute acid, and on the other with solution of table salt. The iron on the side of the acid acts as the positive element, and the other as the negative. This battery which is of considerable power, has a theoretic as well as practical interest.

The other novelty is a battery in which the negative element is chloride of silver fused round a silver wire, the positive element being zinc. The pair are immersed in salt water. This apparatus which is recommended by its simplicity and neatness, is due to Dr. Hugo Müller and Mr. Warren de la Rue.

A curious addition has been made to the materials for the theory of electro-dynamics, in the discovery by Mr. Edmond Becquerel, of the fact that capillarity is an electro-motive power. The contact of solutions in capillary spaces is attended with the deposition of their bases in metallic form, precisely as in galvano-plastic operations. This discovery is eminently suggestive. Its farther prosecution cannot fail to lead to interesting, perhaps to important, results.

To dynamic electricity Dr. Edlund has also contributed the discovery that currents passing through conducting metals produce an expansion of the conductor which is independent of heat.

Certain phenomena observed by Professor Daniell show the existence of a considerable mechanical power in the voltaic current, capable of transporting heavy substances in mass. This discovery taken in connection with nearly simultaneous observations by Mr. H. Poggendorf on an analogous effect produced in the movement of static electricity, presents a paradox of which the solution is not obvious. In the voltaic current a conducting substance like mercury is transported rapidly along a tube from the positive to the negative pole. This movement will even take place on an inclined plane against gravity. In the continuous discharge between the electrodes of a Holtz machine, however, the mercury moves with equal rapidity from the negative to the positive pole.

Discussion still continues on the much controverted subject of spontaneous generation, notwithstanding the seemingly conclusive experiments of Mr. Pasteur. In connection with this subject, Dr. Wyman's investigations with respect to the power of living organisms to resist heat, published since your last meeting, possess unusual interest.

Recent publications throw much light upon the question heretofore unsettled, as to the immediate source of the energy manifested in the muscular contractions of living animals. It has been a favorite supposition that this energy is furnished at the expense of the muscle itself, and that no part of it is derived from any other source. This hypothesis seems to have been conclusively overthrown, by recent observations; among which may be mentioned those of Dr. Douglass upon the prisoners in the Madras penitentiary, which show that the proteine compounds furnished in food are not an adequate equivalent for the work performed. Muscular energy must therefore, in a measure at least, be derived from the oxidation of non-azotized compounds contained in the circulation. But this information, though interesting, brings us no nearer to the solution of the mystery how heat or electricity or chemical force becomes muscular force in the living animal. On the subject of muscular dynamics, there is absolutely no theory at all. We have a rational proximate account of the conversion of electrical energy into magnetic energy. We have a conception of how and why the magnet produces motion. We have a theory also of the transformation of heat into molar force in the cylinder of the steam engine. But in regard to the contraction of animal muscles there is not yet in any proper sense, any physical theory whatever. We have on the one side the chemical action. We have on the other the mechanical. The connecting link—the mode of transformation of the one into the other—is wanting—so utterly wanting that the vaguest conjecture cannot hazard even a hypothesis to supply it. Have we not here probably reached a limit which it will never be permitted to human investigation to transcend? This question suggests a thought to which I desire, for a moment before concluding, to draw your attention, and for the sake of which I have purposely abridged the imperfect sketch I have attempted of the recent progress of science.

The great flood of new truth which scientific investigation has in our own day let in upon the world, the multitude of mysteries of which but lately it would have seemed hopeless, perhaps presumptuous, to attempt the solution, but which have melted away nevertheless in the powerful focus of modern scrutiny, have given birth to a feeling, not yet perhaps universal but quite general, that there is no truth whatever which is not explicable by physical law; or, to state the proposition a little differently, that, whatever may be claimed as truth without being so explicable, is but idle fancy, the mere "stuff that dreams are made of," unworthy to be considered by the genuine philosopher. The doctrine, if I understand it, is that we know nothing but phenomena, that as to the relations of these we know only that they occur in certain invariable sequences, the idea of causation being a mere conceit, or at least a thing with which we have nothing to do; and that whatever transcends observation, or is beyond the reach of demonstration, must be set down as matter of useless speculation, as utterly and absolutely unknowable. Under the operation of this principle, metaphysics in the highest sense of the word, that is to say abstract philosophy, and ontology, or the theory of being, must be swept out of existence. So at first thought, it would seem, must be psychology; but this appears not to be so, and it is precisely here that I find the point to which I propose to direct my animadversion. The spirit of the so called positive philosophy very strikingly characterizes much of modern physical inquiry, even when conducted by men who repel the imputation of belonging to the school of Comte. It is in harmony with this spirit to exact that all phenomena, whether mental or material, shall be regarded as belonging to one common class, and shall be treated in precisely the same way. It thus builds up a new mental philosophy of its own; or reduces rather mental philosophy to be a branch of physics. I believe that this exaction is too comprehensive. I believe that physical inquiry has a field for its legitimate application which is limited by certain natural and definite boundaries. I believe that the positive spirit itself properly applied within this field, leads us directly up to the conclusion that there is something certainly existent which it cannot reach, and which if not absolutely knowable is yet knowable in its most important relations; which is only to say that it is knowable in precisely the same sense in which we can be said to know anything whatever. That I may make myself more clearly understood, I must enter into some detail of illustration.

A very distinguishing peculiarity in the physics of recent years is the prominence which has been given to the doctrine of the conservation of force. A doctrine bearing this name has indeed long been recognized in mechanics, but it was earlier restricted to mechanical force alone; and as to force in that form even, was held to be true only under certain conditions, or with certain limitations. If the motion of a system of bodies is modified by their mutual action on each other, whether they draw each other by means of inextensible cords, or impel each other by means of rigid rods—the connections in either case being without inertia or weight—or whether they attract or repel each other by forces varying according to any law, or whether finally they act by direct collision, provided only they possess the property of perfect elasticity; in any of these cases the sum total of the living forces of all the bodies of the system will remain absolutely the same. This law was announced by Huyghens more than two hundred years ago. At that time force was understood in no other sense than as the power to move visible masses, or as the energy embodied in such masses when in motion. The truth was therefore rather abstract than practical. It admitted of no visible exemplification in terrestrial physics. No known solid possessed the property of perfect elasticity; and no experiments on motion could be instituted except in a medium by which motion is continually impeded. The mechanism of the heavens furnished the only perfect illustration in nature of this very important proposition. Notwithstanding the perpetually varying velocities of the great bodies composing the solar system, the consequence of their mutual actions on each other, and notwithstanding the incessant increase and diminution of living force to which each separate body is alternately subject, the sum total of the living forces of the whole, remains from age to age unaltered. But in all dynamical phenomena which occur at the earth's surface, the truth of the law is masked by the unavoidable attendant conditions. Not even is the proposition of the conservation of force as understood in the time of Huyghens, illustrated in the motion of a single body; still less could its verification be looked for in the action of many bodies upon each other. A single body projected through the air, or rolled along a plane, soon parts with its motion. If in order to reduce the number of resistances, and simplify the experiment to the last degree, rotary motion be substituted for linear, and the experiment be conducted in a vacuum, the result only differs from the former by spreading out the waste over a larger period of time; the body still parts with its motion though more slowly than before. In the case of a body moving in a resisting medium—as a cannon ball for instance through the atmosphere, there may have been some early philosophers who imagined that the lost force still survived in the motion imparted to the invisible substance of the air; but in the experiment in which the sole sensible resistance is the friction of an axle upon its bearing, there is no doubt that all the world, men of science as well as those who claimed no such character, agreed in supposing that force was actually lost. The doctrine of the conservation of force, therefore, although the name originated two centuries ago, was not to the mechanical philosopher of that day what it is to us. It was a doctrine of conservation under favoring circumstances, and not of necessary persistence under all conceivable circumstances. Indeed while the notion of force was restricted to the mechanical energy exemplified in moving masses, it was impossible that the doctrine should be understood in any such absolute sense. For this species of energy, molar energy as it has of late become usual to call it, does disappear, as in the case of the turning wheel and the rolling ball, or as in the collision of bodies destitute of elasticity; so that if molar energy is the only form of force admitted, the doctrine of the conservation of force is not universally true. This doctrine therefore, in the comprehensive sense in which we understand it, is a doctrine of our own time, though it has borrowed its name from another age. The term force has for us a much wider significancy than it had for the mechanical philosophers of the period of Huyghens and of Newton. It comprehends all those influences which were known to early physics by the name of the imponderables: powers concerning the nature of which there have been so many and so discordant hypotheses, which have been conceived of as fluids permeating the pores of the most solid bodies yet without increasing their weight; or as showers of minute projectiles pervading all space, yet without possessing momentum. Heat, electricity, light, are all regarded now as forces into which molar energy when it disappears is completely transformed; and to these we may add gravitation and chemical affinity. Magnetism, which in the books of the early writers occupied so prominent and so important a place, has ceased to take rank as an independent force, but is merged in electricity of which it is one of the manifestations. The facts that molar force may generate heat, and that heat in its turn generates molar force, are facts that have been long enough familiar: the notion even that heat is transformed force is by no means new. It may be said indeed that the experiments of Rumford, at the close of the last century, were sufficiently conclusive to establish this last doctrine. But even to admit this, was not yet to recognize the great truth of the conservation of force in all its fulness; since though it were regarded as true that force is converted into heat, it did not follow that all the force expended underwent this transformation. The water of a stream drives a mill, because the living force of the water in the race is transferred to the wheel; but a large part of this living force escapes without effect nevertheless. A certain mechanical force expended in friction produces heat. Does it any more follow in the one case than in the other, that the heat is the representative of all the force exerted? This could not be asserted until after very careful experimental investigation. Indeed we know that, in this particular form of the expenditure of force, it is not always true; though if the heat produced does not fully represent the force exerted, some other form of equivalent will appear—as for instance electricity.

The doctrine of the conservation of force, understood in the sense of declaring that no force is ever lost and no force ever created, is one of which the truth could only be demonstrated after experimental science had reached a very high degree of refinement. And it could receive no experimental demonstration until there had been established some unit of reference to serve as a common measure of the quantities compared. If a given molar force disappears with the effect of raising the temperature of the body on which it has expended itself, and if the original *vis viva* and the resultant heat are equivalent in quantity, the same amount of force must be competent to produce always under the same circumstances, and in similar and equal masses, the same elevation of temperature, and different forces must produce effects of the same character in proportion to their amount. A satisfactory unit of comparison may therefore be found in the quantity of heat which is sufficient to raise a unit of quantity of standard matter, as for instance of water, one degree of the thermometer. With such a definite standard of comparison, it is possible to test the truth of the hypothesis that heat and force are convertible, and it is in this way that this important proposition has been experimentally established. The same exact equivalency has been in like manner shown to exist between electricity and heat, between chemical action and electricity, and between each of these and molar energy. And as light and heat seem to be but different manifestations of the same force, or different modes in which the same force is related to our perceptions, the law of conservation of force may be presumed to extend equally to both, and may therefore be regarded as the great law underlying and controlling all physical phenomena.

It has been affirmed, as I am aware, by a very distinguished metaphysician, Mr. Herbert Spencer, that this important truth is one which needs no demonstration. It is, as he asserts, a truth, which has its origin in direct intuition, and which lies at the foundation of all the knowledge we possess of the material world. According to him, indeed, we have no knowledge whatever which is not traceable back at last to force; and as to force he tells us that what we know of it is only, first, that it *is*, and secondly that it persists. In regard to the persistence of force, he finds the evidence of this intuition in the universal recognition of the postulate that action and reaction are equal.

This allegation, he holds, is but another form of saying that there cannot be an isolated force beginning and ending in nothing; but that any force manifested implies an antecedent force from which it is derived and against which it is a reaction. Clearly then he says, the persistence of force is an ultimate truth of which no inductive proof is possible. This exposition of the postulate is not perhaps entirely convincing. If reaction can be only understood as a taking up in living form by one body of a force which another body abandons, then Mr. Spencer's interpretation of it is true. But if it can also be understood as a destruction of any part of the original force, the consequence he deduces from it does not follow. To impute to it the former sense exclusively, is to reduce the postulate to an identical proposition, which in its proper form of expression would be this: Force which persists, persists. The truth is that this proposition, and all the laws of motion laid down by Newton, as well as the doctrine of the conservation of living force enunciated by Huyghens, were understood when announced, and long after; to be true only of forces acting in circumstances which are never realized at the earth's surface, and as theoretic and not as practical laws. They were admitted as deductions from observation, and not as self-evidently necessary. As we now understand the subject, Newton's third law is literally true under all circumstances, and it does in fact draw after it the necessity of the persistence of force; but this is a very different thing from supposing such a necessity to be a truth of intuition. The question of the origin of this notion is however of no practical importance: for the admission of the truth of the conservation of force as a theory does not render it any the less necessary that experiment should come in to inform us what are the quantitative equivalents of force under its various forms; and without a knowledge of these equivalents the doctrine can admit of no useful application as an aid to investigation.

I should not be justified in detaining you so long over a truth which in the physics of our day has become elementary, were it not that the bold extension which has recently been made of this law to the phenomena of conscious life, seems to me to transcend the limits which must ever arrest the progress of successful physical inquiry.

Besides the forces which we have been considering, and which are the only forces whose existence is made known to us in the phenomena of the inorganic world, there is another, whose effects are only seen in bodies endowed with life. Perhaps it should rather be said that there is a class of such forces (I call them forces for want of a better term) for the manifestations assume several very distinct characters; yet though the results are exceedingly conspicuous, the mode of operation is so obscure as hitherto to have baffled investigation. Under the influence of these forces compounds are built up which chemical art has in vain attempted to imitate, and which are of so noticeable instability that they break up spontaneously directly after the sustaining power is withdrawn. The bodies formed of these materials commence usually in germs of exceeding minuteness, pass through a regular cycle of growth and decay, and are at last abandoned by the mysterious principle which has given them their form and their structure, and left to perish. Among these bodies which make up the empire of organic life, two broadly distinguished kingdoms present themselves, the vegetable and the animal. Both of these are equally animated by the force which determines organization; but the former except in this particular do not differ from the brute matter out of which they spring; while the latter possess the power of self movement, and a will which directs this power. They are moreover influenced in their movements by impressions made upon them from without; impressions which we call sensations and refer to a property which we denominate sensibility. If we consider the animal kingdom more in detail, we shall see that it embraces classes widely differing from each other in point of dignity. Voluntary motion and sensibility in its lowest form of manifestation—the sense of touch—seem all that distinguish the humblest of these classes from vegetables. As we ascend in the scale, other senses appear, and a distinct intelligence with the evident exercise of judgment and memory displays itself; while the manifestations of emotion, as of pleasure, apprehension, anger, etc., are very obvious. These mental and emotional qualities increase in the energy of their development through many successive grades, until at length, leaping a great gulf of division which separates the highest order of animal life from all other forms, they blaze out in man in the full effulgence of a perfected reason. Nor is it only the mental superiority of the human race which gives it its supremacy in the animal world. To deduce consequences from premises, to follow out a chain of causes to its probable results, is a high endowment, but it is one which is partaken though doubtless in a degree infinitely inferior, by many animals. But to distinguish between right and wrong — to conceive even the notion that there is a right and a wrong, argues the possession of an entirely new faculty. It matters not upon what basis ethical writers may choose to build up their systems, denying often the existence of such a thing as an innate moral sense. It matters not by what means they may explain the genesis of the ideas of right and wrong as originating in conceptions purely intellectual — the fact remains that man has a sense of right and wrong which is sustained by a faculty in him which he calls conscience, — a faculty which he knows to be a very different thing from simple understanding: and no evidence has ever yet appeared that such a sense or such a faculty exists even in the most embryonic condition in any other living animal.

We have then here several different forms of force associated with life: the organizing force; the force represented by sensibility; the force of will; the force of intelligence; the emotional force, and the force which manifests itself through the moral sense. If to all these principles I apply indiscriminately the word force, it is partly because of the poverty of language which furnishes no other generic term under which to embrace them all; partly because one of them at least, the organizing force, has long been distinguished by that name, though its existence as a physical force has been generally denied; and partly because when it is claimed, as it is of late claimed, that sensation, thought, conscience, emotion and will, are only manifestations of known physical forces acting upon the living organism, and are in fact those same known forces presented under new forms, these functions of sentient and intelligent life are by the terms of the proposition itself reduced to the class of physical forces and are properly called by the same name.

The question is, are we compelled to concede this claim. In accepting the doctrine of the conservation of force have we no choice but to admit that the barrier so long supposed to exist between the spiritual and the material world is a sheer creation of the imagination, and that the mind is as literally a machine operated by strictly physical forces as a wind-mill or a steam engine? Such an admission is by no means flattering to man's self-esteem. It is nothing less than revolting to his religious instincts. What are the arguments on which we are expected to accept it?

In regard to the processes of organic growth and development, of the assimilation of food, the absorptions, secretions and other physiological functions continually in activity in the living animal or vegetable, it may be admitted without hesitation that the efficient force producing the corresponding changes is derived from without, and that the changes themselves are the strict equivalents of the force thus expended. Take the case of the growing plant. Its food is mainly carbon, derived from the carbonic acid of the water which it draws up from the earth, or finds in the air around it. The force by which this very stable compound is decomposed, so as to leave the carbon free to combine with the plant, is furnished by the sun's rays. Of the equivalency of the two forces there can be no doubt; nor have we any need in the case to call in any special force under the name vital, to perform any part of the work of the change. And yet we certainly have need of something more than the mere juxtaposition of the materials in presence of the solar influence to produce the effect. If the principle of life is not in the plant, the operation will not proceed. The carbonic acid may be there, the vegetable tissue may be there, and yet the solar rays may play upon them forever without producing the slightest effect. The vital principle then is the something which causes the plant to grow. I will not call it a force—I think the term vital force a misnomer—because there is no work done for which we have not other forces in full equivalent—but I say that whatever it is, its presence is a necessity to the performance of the work, and in its absence the work is not performed. More than that, I say that not only will not the forces which produce growth during the life of the plant, be sufficient to do so after life has ceased; but no combination of forces or influences or materials which human skill can contrive can now or will ever produce one leaf of the simplest plant or one blade of the humblest grass of which nature under the influence of the principle of life is producing millions on millions every succeeding summer.

More than this, there is a peculiarity of the compounds which are formed during the growth of organized beings, animals as well as plants, which marks a wide departure from the mode in which the forces of nature act when left entirely to themselves. The tendency of these forces is to equilibrium. Upon an irregular plane a heavy body, unobstructed in its motion will seek the lowest level. A balance, when disturbed, may oscillate for a time, but it comes at length to rest. Waters make their way to the ocean. Unequally heated bodies placed near each other share at length between them the common stock of heat. Electricity accumulated in excess upon one body will escape to others less highly charged. So too of chemical forces. They tend ever to produce the most stable compounds. A stronger combination never spontaneously breaks up to give place to a weaker. Suppose that all the materials of which the earth is composed should be thrown together in elementary form, it is possible that by the first action of affinities many feeble combinations would be produced; but it is certain that these must one after another give way to stronger ones until the whole should be combined in forms possessing the absolute maximum of stability, unless the process should be arrested by a solidification of the mass preventing farther motion. Now in the compounds produced during the growth of plants and animals, there is a complete inversion of this process: that is, there is an ascension from the lower to the higher level—a substitution of the weaker for the stronger, of the unstable for the stable. And animal compounds, that is those formed where the type of life is highest, are, as a rule, greatly more unstable than vegetable. The presence of the principle of life in organized bodies, therefore, determines the physical forces which in such bodies do the work of change, to operate in a manner in which they do not operate where it is absent. Light and chemical affinity, for instance, are interchanged in the plant. Light disappears, and in disappearing restores to activity the force of affinity which was dormant because satisfied, but in order to do this it needs a determining influence which it finds in the simultaneous presence of the vital principle, and never without. Nor need it be said that in exercising such a determining power, life becomes a form of physical force. The light consumed and the chemical force revived are complete equivalents. Each represents the other. Neither has derived any increase nor suffered any diminution from association with the vital principle. What this principle has done is to determine, in regard to two forms of the same force possible in the same organism, which shall be the form manifested and which the form suppressed.

But if we are not to regard this influence of the vital principle as of the nature of a force, it will perhaps be demanded how it shall be explained? That, I reply, is a question which admits of no answer. It is a question which cannot be answered now, and a question which will not be answered ever. It is an inquiry which leads us beyond the limit of legitimate philosophical research. The vital principle differs from every form of force known to us, and from every other known property or quality, in that it confers upon the body which it animates a special character of individuality, and in that it is incapable of being insulated or of being transferred from body to body. We know it only through the peculiar organizing power which belongs to it, and which is manifested not merely in the chemical changes which it determines, but in the external forms which the resulting compounds assume.

The phenomena of vegetative life present us then with an inscrutable mystery, but they suggest no necessary conflict with the great doctrine of the conservation of force. But when we advance to the higher manifestations of life we are told that such a conflict does threaten us, unless we yield up all that we have been taught to believe of the possibility of spiritual existence, and relinquish, as untenable, convictions which have been partaken by the whole race of mankind. Organic changes are physical effects, and may be received without hesitation as the representative equivalents of physical forces expended. But sensation, will, emotion, passion, thought, are in no conceivable sense physical. That they may be excited by the impressions of physical forces upon the animal organism is indeed admitted; that they are so is a matter of common experience; but that they are these forces themselves transformed into consciousness, and that in ceasing to be what they are they become physical forces again, is a supposition so totally repugnant to our instinctive convictions, that at the first announcement one is at a loss to conceive how it would ever have been for a moment entertained. This however is a philosophy which at the present day is boldly taught in public schools of science, and which numbers among its disciples many very able men. One of its most distinguished teachers and oracles, Mr. Herbert Spencer, sums it up briefly and tersely in these words:

"Various classes of facts thus unite to prove that the law of metamorphosis which holds among the physical forces holds equally between them and the mental forces. Those modes of the unknowable which we call motion, heat, light, chemical affinity, etc., are alike transformable into each other and into those other modes of the unknowable which we distinguish as sensation, emotion, thought: these in their turn being directly or indirectly retransformable into the original shapes. That no idea or feeling arises save as a result of some physical force expended in producing it, is fast becoming a common place of science; and whoever duly weighs the evidence will see that nothing but an overwhelming bias in favor of a preconceived theory can explain its non-acceptance. How this metamorphosis takes place, how a force existing as motion, heat or light, can become a mode of consciousness—how it is possible for ærial vibrations to generate the sensation we call sound, or for the forces liberated by chemical changes in the brain to give rise to emotion, these are mysteries which it is impossible to fathom. But they are not profounder mysteries than the transformation of the physical forces into each other. They are not more completely beyond our comprehension than the natures of mind and matter. They have simply the same insolubility as all other ultimate questions. We can learn nothing more than that here is one of the uniformities in the order of phenomena."

Nothing need be added to the explicitness of this statement. In every school of philosophy, the union of mind with matter has been among the profoundest of mysteries. There have been teachers who have held that mind itself is material, but even such have not assumed that thought is matter also. Thought has been regarded as a property, an endowment, a quality of the matter of which mind is made. The modern school which inculcates the dogma that mind is force, is no less materialistic than that of Hobbes or Spinoza. We know nothing whatever of force but as a concomitant of matter. Without the existence of matter external to us, we should know nothing of force. Mr. Spencer, indeed, regarding the question from an opposite stand-point, lays it down among his fundamental propositions, that without the existence of force we should know nothing of matter; but his very illustration of this proposition shows its fallacy, or at least the very equivocal nature of the evidence on which it rests. For the force of which, as he correctly says, we first become conscious, is the force of resistance, perceived when the organs of touch encounter a material substance. Yet no such resistance could be felt unless the organ itself were material, and hence it is the preëxistence of matter which is the indispensable condition of the discovery of force. The two ideas in fact originate together. Resistance discovers to us the existence of matter and of force simultaneously; and hence it is that we may reaffirm that we know nothing of force and can conceive nothing of force except as it is a concomitant of matter. The force of resistance is a dead force. Living force is a property of matter in motion. In those forms of the unknowable, so called by Mr. Spencer, which we name heat and light, modern philosophy recognizes the motion of matter, no less than in the energy of a projectile thrown from a cannon, or of a planet revolving in the celestial spaces. When heat was recognized as a force it was declared to be a mode of motion, and this descriptive title was by common consent received as fit and proper. Light, electricity, chemical action, are regarded no less as modes of motion also. In the case of these unknowables, the motion is presumably molecular, and in contradistinction to this, the force of moving masses is called molar; but in the case of all known forces, however unknowable they may be and however unknowable as to their essential nature they are in their mode of efficiency, they are simply matter in some mode of motion. The philosophy therefore which makes thought a form of force, makes thought a mode of motion; converts the thinking being into a mechanical automaton, whose sensations, emotions, intellections, are mere vibrations produced in its material substance by the play of physical forces, and whose conscious existence must forever cease when the exhausted organism shall at length fail to respond to these external impulses. If the law of conservation of force is therefore to be extended to mental phenomena, the immortality of the soul can be no longer maintained. On this hypothesis indeed man has no soul. Life is but a momentary phenomenon, a casual condition of matter, to be classed with combustion, incandescence, sound, odor, anything most accidental and evanescent. More than this, the living being, while his brief consciousness endures, is the mere sport of forces foreign to himself. His conscious freedom of will is nothing but an illusion. His thoughts, his feelings, his acts, are all links in a chain of inevitable events, determined by unalterable physical laws. He ceases to be a moral agent or an accountable being. Let it be observed moreover that the doctrine of necessity here forced upon us, differs from that which has been inculcated by necessitarian philosophers heretofore. The necessity which fetters the will by making it the slave of motive, is one in which the coercion is moral and not physical; and which leaves us room at least to respect poor human nature though we may compassionate. That which presents will and motive together as two modes of motion, of which the first is but the second under a new form, is a necessity of which the slave has lost even respectability, and is reduced to the humble level of a piece of mechanism.

But it is not because of these consequences that I reject a doctrine so derogatory to the dignity of humanity. The business of the philosopher is to follow on the trace of truth whenever it may lead. He must not suffer feeling, or preference, or prepossession, or prejudice, for a moment to bias his judgment. If the law of the conservation of force, rightly interpreted, conducts us of necessity to materialism, we must accept the conclusion, however humiliating we may find it to our pride or however ruinous to our hopes. To my mind no such necessity exists.

In proof of this position it is of course no argument, and I do not present it as one, to allege the utter incongruity which every mind uninfluenced, I was about to say unperverted, by favorite philosophical preconceptions, feels to exist between mental and physical phenomena. Consciousness, to any mind, is a mystery sufficiently profound; but to suggest that it may be only a mode of motion, is to an ordinary mind little less than a self evident absurdity. But, as just remarked, this is no argument.

It is an argument, however, to say that thought cannot be a physical force, because thought admits of no measure. I think it will be conceded without controversy that there is no form of material substance, and no known force of a physical nature (and there are no other forces) of which we cannot in some form definitely express the quantity, by reference to some conventional measuring unit. Even while heat and light and electricity continued to be regarded as independent and unconvertible forces, they were still subjected to measurement, each after a manner peculiar to itself; and the ratio between the amount of latent heat of a pound of water and that of a pound of steam was as well known as it is now. The minutest quantities of electricity were determined by the most delicate of balances, and the intensities of different lights were numerically expressed by means of experimental and instrumental comparisons. Now no such means of measuring mental action has been suggested. No such means can be conceived. It is not a sufficient reply to this difficulty to say that thought is unlike any other species of quantity known to us, and therefore no such species of quantity can furnish us with a measure. We do not ask that it shall be measured against anything unlike itself. Heat was not originally so measured nor electricity nor light. A unit for the quantitative comparison of each of these powers, was found in a determinate quantity of the thing itself which was to be measured. And before thought can be claimed to be a form of physical force, it must become possible so to define a given quantity of thought, that all mankind, or if not all mankind, every scientific investigator at least, shall be perfectly able to understand exactly what quantity is meant. Can this be done? If so it will probably be in the same way in which units of reference have been established for other powers. Such units have been fixed by considering intensity, time of action, quantity of matter acted upon, velocity generated, or work done; or by a portion of these elements variously combined. The force of gravity is measured by quantity of matter, time and velocity. Heat is measured by quantity of matter and determinate elevation of temperature. Light by intensity at a determinate distance. Now no one would think of measuring thought by the effect it produces on matter external to the thinker, which is *nil*. Of all the possible indications of greater or less of which the case admits, there are but two—intensity and time of action,—which are not self-evidently, at the very first suggestion, unavailable. Nor would these two alone suffice for a standard of *general* reference, even were the force one unquestionably susceptible of measurement; since quantity depends not only on intensity and time but also on the magnitude of the source. For a given individual this consideration might be disregarded; and the quantity of the *thought-force* of such an individual might be measured by the intensity conjoined with the time, but this only on supposition that thought-intensity is susceptible of estimate. On the impossibility of such an estimate it is hardly necessary to dwell. Now I maintain that a thing which is unsusceptible of measure cannot be a quantity, and a thing that is not even a quantity cannot be a force. I know it may be replied that though the unit of measurement may not yet have been found, it may nevertheless be so hereafter. I answer no; the objection is not merely that such a unit is at present impossible, but that even the conception of such a unit is an impossibility. Not only is the quantity of thought in a human brain presently unmeasurable, but its measurability is not even conceivable. And yet if thought is physical force, the time must come when its absolute energy at any given moment, and in any given individual, shall be expressible not only in units of the same description of force, or thought-units, but equally in equivalent units of any other force; so that perhaps we may be able to say of the mental labor of a philosopher in his study, that in half an hour it amounted to fifty or five hundred thousand foot-pounds.

Perhaps it may be said that there is an indirect way of arriving at the unit of thought-force which I have overlooked; that this force is doubtless generated by the oxidation of the brain, that the amount of this oxidation may be ascertained during a determinate period of continuous mental labor; that this oxidation is a chemical action capable of producing a calculable amount of heat, and that this amount of heat may be properly accepted as the measure of the thought-force. This notion will not bear examination. Oxidation is going on at all times in all parts of the body. It is necessary to the maintenance of the animal heat. It is necessary to furnish muscular force. It is necessary to the large involuntary movements such as those of the heart the arteries and the intestines, and those smaller movements which accompany the various secretions. It is necessary especially in the brain to supply the nerve force, whatever it is—probably electricity—by means of which the mind transmits its mandates to the members, and through which it receives the impressions made upon the organs of sense. To distinguish between these various sources of consumption would be a problem of the most complicated character; and the difficulty or impossibility of securing adequate data for its solution by the observation of functional changes in the living man places it practically beyond the reach of investigation. But while this difficult problem remains unsolved, to assume that *if* solved it would show an outstanding balance of force to be placed to the credit of mental action is simply a *petitio principii*, and has no title to respect as an argument. The very strong probability is that no such balance would appear.

It is asserted, however, that some mental impressions are in a manner measurable; that this is true at least in regard to sensations. The feelings of pressure produced upon us by heavy bodies are greater or less in intensity according as the bodies are heavier or lighter in the balance. Bells, vibrating strings, wind instruments, yield sounds which we perceive to vary in loudness according to the degree of force expended in producing them. Bodies at different temperatures, as shown by the thermometer, affect us more or less forcibly with the sensation of heat. To all which it may be replied that sensation and thought are two things essentially distinct, and that we are speaking here of thought and not of sensation; that sensation is an affection of the organ of sense as well as of the mind and is not exclusively mental: that the mental part of it is a cognition of the state of the organ, as modified by a physical force; and that the cognition embraces of course not only the fact but in a general way the degree of the modification; and therefore that a certain quantitative relation between sensation and external force is naturally to be looked for, and in no manner whatever sustains the hypothesis of a conversion of the force into the sensation. As the mind estimates quantitatively a phenomenon of the external world of which it receives information through the sense, so in sensation itself it estimates quantitatively a phenomenon equally external to its own essence though occurring within the material organism with which it is connected. It may furthermore be added that sensation is not, like thought, a thing of which the idea of quantitative measurement is in all cases inconceivable; for though the organ of sense may appreciate differences imperfectly, it s sometimes not very difficult to contrive modes of fixing a determinate unit of measure. This is obviously practicable in the case of sound; it has been actually done in the case of light. Sensation is produced by external disturbing forces. It is attended no doubt with corresponding internal changes. The case is analogous to that of the organizing process, or that of the building up of the structure of the animal or plant. The interchange which takes place between the external and the internal forces takes place without loss or gain to either; but it takes place in a manner which is determined by the presence of the vital principle; and to the mystery which attends the simpler process of organization, it here superadds a new and still more perplexing mystery, the phenomenon of consciousness.

But I proceed farther to say that purely mental impressions cannot be transformations of physical forces, because the character and the intensity of the impressions are not determined by, nor in any manner proportional to, the nature or the amount of the force impressed. The most powerful mental impressions are those which succeed to impulses affecting the nerves of sight or hearing; and these are sufficient often to call into activity all the muscular force which the percipient is capable of exerting. Such impulses, considered as forces, are too feeble, even when most energetic, to admit of being expressed as finite quantities. Yet if we interpret the phenomena in strict accordance with the hypothesis under consideration, we must say that these impulses are first transformed into mental force, and that afterward this mental force, by a new transformation, takes the phase of muscular and finally of molar energy. Thus understood the doctrine is manifestly absurd, since it makes a small force equivalent to a large one. To evade the difficulty, its advocates introduce here a new and supplementary hypothesis, according to which, while the mental impression is the equivalent only of the external physical force, the more palpable manifestations which follow come from the chemical action excited in some confessedly incomprehensible manner, by the instrumentality of the mental impression. But this explanation is totally unsatisfactory, and is hardly less than fatal to the hypothesis it is intended to subserve. Whatever claim to acceptance the new theory possesses must be founded on the promise it holds out of a solution of a mystery which the spiritual theory candidly recognizes as above solution. This promise it fulfils only by presenting the original mystery under a novel form. How to explain the connection of mind with matter is the problem ostensibly proposed; and a solution is assumed to have been furnished when mental forces have been reduced to the level of physical. But how to explain the influence of the will over the members, or the power of emotion over the muscular system, this is the true difficulty; and this the solution offered does not reach in the least. We all admit that the sensible displays of force of which animals are capable, are derived from the chemical action which takes place in their organisms, and are examples of the transformation of one form of force into another in conformity with the law of conservation. But the new philosophy does not pretend that *these* forces are the equivalents of the mental forces which have set them into activity; or that they have either wholly or partly passed through the form of mental force previously to their appearance in the mechanical form. How then does the mental force—that is, the will or the emotion—excite them to activity? That is the question of which we had a right to expect a solution when we were told that the doctrine of the conservation of force was to clear up for us all the difficulties of mental science. And that question is left precisely where it was before.

An analogy I know has been intimated between this case and that of the explosion of a mine by the application of a match, or the discharge of a gun by the pulling of a trigger—examples in which a very slight force may suffice to evoke another of tremendous power. This argument would perhaps be entitled to some respect, if an attempt had been made to point out any conceivable mode in which, in the case in hand, the match is applied or the trigger is sprung. In the absence of any such attempt, it amounts to nothing more than pure hypothesis. It is simply another form of asserting that the connection of the phenomena is physical because it *must* be physical. It is once more a naked *petitio principii*. We have here a repetition of the difficulty which presents itself in the case of vegetable growth. In the presence of the principle of life there occur sequences of phenomena which do not occur in its absence. The facts are undeniable, but a rational explanation of them is as far off as ever.

Another consideration here presents itself which is of profound significancy, and which to an unbiassed judgment can hardly fail to be decisive. Mental impressions excited by impulses affecting the nerves of sense, take their character and are determined in their intensity not in the least by the force which those impulses represent, but by the *ideas* momentarily associated with them. An insulting expression addressed in English to a Frenchman unacquainted with that language will be heard with indifference, while an Englishman in the same circumstances will be roused to indignation. Any man may contemplate a dagger in the hand of a friend as coolly as he would do any other piece of cutlery; but if he sees the same weapon uplifted over him by an assassin, he will be filled with alarm, and will instantly put forth all the strength of which he is possessed, for the purpose, if not of resistance, at least of escape. In these examples the physical forces in the cases contrasted are as nearly equal as possible; but the mental impressions which they produce are widely different. There is no explaining these discrepancies consistently with the hypothesis under consideration. There is in either case but a given amount of external physical force; there can be only the same definite equivalent of internal mental force. In the first instance the collocation and sequence of impulses on the ear of the Englishman conveys a *meaning* which in the case of the Frenchman is wanting, and the mental impression is consequently more powerful. Shall we say that this meaning—this accident accompanying the sounds—is force also? If so, how is it projected into the mind of the hearer? And how into one mind more than into another, when the physical impulses are in both cases identical? Shall we find any help in this difficulty from the doctrine of the association of ideas? What is the association of ideas? Is it anything more than a *habit* of consecutive occurrence, acquired by the frequent juxtaposition of the ideas in the order of time? And can unconscious forces be trained to form habits of diversity of action under physical conditions entirely similar? Of course there is no escape from this difficulty, but by assuming, as the advocates of this theory do assume, that unconscious forces become forms of consciousness; but this assumption involves the inevitable consequence that, in undergoing this transformation, they cease to be distinguished by that invariability in their modes of action which is the distinctive characteristic of physical force. Consciousness indeed is the rock on which this theory splits. Its supporters make no effort to explain this marvellous phenomenon. They admit, and indeed proclaim, that no explanation is possible. We make the same admission, but we claim at the same time that it is clear enough what it is not. The very fact that we can trace an unbroken series of entirely intelligible effects all the way up to the very point at which this surprising phenomenon presents itself, and then suddenly lose the thread altogether, is sufficient evidence that it is not physical.

But I will not pursue the argument. No possible *explanation* of mental phenomena can be founded upon a hypothesis which attempts to identify them with physical forces; but on the other hand the attempt leads inevitably up to the conclusion that there is something existent which is beyond the reach of scientific investigation—something of which, nevertheless, the existence is just as certain as its nature is inconceivable.

It is the fashion, I know, in the school of the positivists, to treat as unreal whatever is undemonstrable. But the positivist believes in force, although he cannot tell what it is. And the organic world furnishes just as conclusive evidence of the existence of an influence superior to force, as the physical world exhibits of the existence of force itself. If indeed in accepting the doctrine of the identity of mind and force we could rid ourselves of mystery, we might find in such a fact some plausible argument for insisting on its admission. But in this respect it fails to advance us a single step. When it is objected that we can form no conception of mind, we reply that it is just as much out of our power to conceive of matter. If it is an inexplicable mystery that mind should act upon matter, the action of matter itself upon other matter is a mystery just as profound. It is certain that no two atoms touch each other. How then can many atoms cohere to form a solid mass? Even the great intellect of a Newton confessed itself baffled in the endeavor to conceive in what manner the sun could influence the planets, acting through a vast void space measured by millions of miles. Yet precisely the same difficulty returns in regard to every two particles which concur in the formation of the minutest material substance. It was a conception of Dr. Young that if one hundred men should be distributed equally over the surface of England, the distances between them might not be more disproportioned to their own dimensions than the void spaces in any solid to the atoms composing the solid. Does it then explain in any manner how mind acts upon matter, to say that mind is force—that is, that it is matter in motion?

The reply may be supposed, that the action of matter upon matter is a fact of observation; and that we must therefore accept it as a fact, and because it is a fact, although we cannot comprehend it. But the action of mind upon matter is equally a fact of observation, and not only that but a fact of consciousness also. Thus we gain nothing whatever even in the way of simplifying our philosophy, by contradicting our intuitions, resisting our instinctive convictions, and abjuring our faith.

In what I have said it has not been my design to obtrude upon the Association any theological dogma, or to advocate any religious creed. With such subjects this Association as an Association has nothing to do. I have not indeed, here or elsewhere, been in any manner instrumental in introducing questions of religion or of abstract philosophy into discussions of physical science. But when such questions are introduced by others, and when it is demanded of us to pronounce as physicists that spiritual existence is an absurdity and religion a dream, it seems to me that no choice is left us but to proclaim our dissent, or be understood by our silence to accept the doctrine as our own. When such is the alternative, for one I feel bound to speak, and to declare my conviction that as physicists we have nothing to do with mental philosophy; and that in endeavoring to reduce the phenomena of mind under the laws of matter we wander beyond our depth, we establish nothing certain, we bring ridicule upon the name of positive science, and achieve but a single undeniable result, that of unsettling in the minds of multitudes convictions which form the basis of their chief happiness. If my views are correct, there is certainly a field which it is not the province of physical science to explore; and which, if we are wise, we shall carefully refrain from invading. Either this is so, or man himself is but a transient unmeaning phenomenon, brought into existence without a purpose and without a destiny; and neither science nor any other human interest is worth pursuing. In conclusion, gentlemen, thanking you for the kind attention with which you have listened to me, permit me to congratulate you on the cheering auspices under which you are once more assembled. You are here in a strength which recalls the happy days when your Association was in the zenith of its prosperity and its usefulness, and which justifies the hope that a fresh career of still more fruitful labors and of higher services to humanity is before it. May your mutual intercourse be productive of all the gratification which you have anticipated, and may the interchange of views in which you are about to engage bring with it at the same time enjoyment and profit.

PROCEEDINGS

OF THE

CHICAGO MEETING, AUGUST, 1868.

COMMUNICATIONS.

A. MATHEMATICS, PHYSICS, AND CHEMISTRY.

1. On the Application of Electricity to the Maintenance of the Vibrations of the Tuning-fork, and of the Tuning-fork to the Excitement of Vibrations in Cords and Threads. By Joseph Lovering of Cambridge, Mass.

The application of electricity to maintaining the vibrations of the tuning-fork is not entirely new, though it is of recent discovery; and the use of tuning-forks in producing sympathetic vibrations in threads or cords is not new, though it is of recent discovery. The tuning-fork, now exhibited to the members of this Association, was made from a bar of iron three-eighths of an inch in thickness, two inches in width, and sixty inches in length. Having great mass relatively to its surface, it is able, even without the help of electricity, to preserve its motion for several minutes, unchecked by the resistance of the atmosphere, and is, therefore, peculiarly fitted for the purpose of communicating corresponding vibrations to attached threads or cords. Nevertheless, these vibrations are slowly dying out. The length of the magnet is horizontal, the plane which passes through the two prongs being vertical. A magnet of the form of the letter U is so placed, that one of its branches is above the upper prong of the fork, and the other branch below the lower prong of the fork. This magnet consists of a soft iron core, wound with covered copper wire. When a current of electricity is sent into the wire, the iron is magnetized and attracts the prongs of the tuning-fork. If the current acted constantly, the magnetism would retard the prongs when approaching as much as it accelerated them when separating from each other. This retardation is prevented by interrupting the electrical current and suspending the magnetic action when the prongs are approaching one another. This is accomplished by making the longer prong of the tuning-fork a part of the circuit. The upper surface of this prong has a little platinum or silver plate near its extremity, which is just touched by a platinum wire, attached to a spring and supported by a pillar, when the tuning-fork is at rest. After the prongs have moved outwards, the platinum point leaves the plate, the current is interrupted, and the prongs spring together without any retardation. A Grove's battery of six elements arranged, two for quantity and three for intensity, is sufficient to start the tuning-fork when at rest, and to put it in such energetic vibration that the extremities of the prongs strike the poles of the magnet, though at the distance of three-fourths of an inch, when the tuning-fork is at rest. Let one end of a cord be attached to either prong of the tuning-fork, while the other end passes over a pulley and carries a weight of sixteen ounces. If the length and diameter of the cord are such that, with this tension, its rate of vibration agrees with that of the tuning-fork, it will be thrown into a powerful sympathetic vibration. If the weight is reduced to four ounces, the cord will be tuned to the next octave below the note of the fork. Its second harmonic, which is always an octave higher than the fundamental note, will now correspond to the note of the fork. Hence the cord still responds by breaking up into two segments, with a node in the middle. Change now the weight to one ounce and three-fourths. This is about one-ninth of the original weight. Hence the cord will vibrate only once while the fork makes three vibrations, so that there is a difference of twelve notes between them. If the note of the cord were C, that of the fork would be G in the next higher octave. Now it happens that the third harmonic of a cord is always twelve notes higher than its fundamental tone; therefore, the fork may call out a response from the cord, which divides into three segments, and gives forth *visibly* its third harmonic. In a similar way, a tension of one ounce makes the string divide into four segments, by reducing its fundamental note by two octaves. A tension of one quarter of an ounce makes the string divide into eight segments, by reducing its fundamental note by three octaves. A heavier cord is also sometimes used, requiring a weight of one hundred and forty-four ounces when it vibrates in one segment, which makes the phenomena still more conspicuous. With either cord, if the tension is not such as to bring some one of the harmonics into unison with the tuning-fork, the cord is *visibly* silent.

These experiments are particularly valuable as illustrating two propositions.

1. They furnish an excellent exemplification of the great mechanical results which originate in sympathetic motions, when bodies, which stand in each others neighborhood, are nicely attuned to one another in their rates of vibration. Bridges are endangered by the passage of troops when marching to the regular beat of music, and buildings are weakened or destroyed sometimes by the *well-timed* motion of the machinery. It is well known in the factories of Lowell, that the walls will vibrate when the machinery goes at a fixed rate; but if the speed is increased or slackened, they stand perfectly still. In one instance, water was nearly emptied out of a pail by the agitation of the floor on which it stood; bnt when the cause was discovered the remedy was a simple one, viz.: to alter the rate at which the machinery moved.

2. These experiments are valuable, in the second place, as illustrating the *optical* method of studying the laws of Acoustics. In this view of the subject, the paper now presented and the experiments which accompany it, may be considered as a continuation of the paper and the experiments which I offered at the last meeting of the Association. At the Burlington meeting, I discussed the optical method of studying the laws of vibration of *columns of air*. The advantage of the *optical* method of studying *sound* is great, as it makes the experiment independent of the musical excellence of the ear of the experimenter, and relieves it of a large *personal equation* in the observer. Any person, by the use of this method, may accom-

plish more, with the *coarsest vision*, than would be possible, by the ordinary methods, to the most highly cultivated musical *ear*.

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2. Laws of the Ocean Currents. By J. S. Grimes of New York.

PRELIMINARY EXPLANATIONS.

1. The prime moving cause of all the constant ocean currents is the difference of temperature between the warmer and colder latitudes. The surface water in the colder latitudes becomes cool and condensed so as to sink and flow in deeper currents towards the equator, while the surface water in the tropical latitudes is warmed and overflows towards the poles. These two currents continually balance and compensate each other. Were it not that the earth turns daily on its axis from west to east, no farther explanation of the ocean currents would be required.

2. It is now well known to geographers that when a current flows from the equator the rotation of the globe causes it to flow easterly, that is it flows north-easterly in the northern and south-easterly in the southern hemisphere. When on the contrary a current flows towards the equator the effect of rotation is to deflect it in a westerly direction — that is south-west in the northern and north-west in the southern hemisphere. This also is very simple, and is a sufficient explanation of all the currents of the ocean excepting five. There are five great elliptical currents, one in each of the oceans, that require farther explanation.

3. There are then two classes of ocean currents, the local and the elliptical. The local currents I have already described. They move either north or south and do not return, but are lost in the masses of the ocean. The elliptical currents have not hitherto been understood; indeed, it is only within a few years that their very existence as a class has been recognized. According to the best information that I have been able to obtain, Professor Joseph Henry, of Washington, was the first to announce the fact that in each of the five great oceans there is one vast whirl, or elliptical current. His article was pub-

lished in one of the patent office reports about ten years ago. Professor Dana in his excellent Manual of Geology not only makes the same statement, but illustrates it by an engraving.

The only hypothesis that has been offered to account for these whirls or ellipses, is that they are owing to the basin-like forms of the depressions in the earth's crust in which the ocean is contained. If we do not scrutinize this hypothesis too closely, it seems quite plausible. The warm currents move north-east (in this hemisphere) and the cold currents south-west, and both being forced against the sides of the basin, are driven entirely around it. I say this seems plausible; but when we apply the *experimentum crucis* the hypothesis fails, for in repeated instances the currents leave the shores or sides of the basins and flow off in tangents to pursue their own elliptical orbits; or, if like planets, they are impelled by an invisible and irresistable power, over which neither the shores nor the winds have any essential influence. Observe, for instance, the elliptical current of the North Pacific, leaving the coast of California to flow south-west, while the coast trends to the south-east. See the same current in mid-ocean before it reaches Asia turning north-west. Here is no land nor basin sides to deflect the current.

Again, in the South Pacific, the current flows from the Antartic Sea north-east to Chili. Afterwards it leaves the coast of Peru and flows south-west, and then west, and while flowing in mid-ocean it turns again and flows south between Australia and New Zealand. In the South Atlantic observe the current flow from the coast of Brazil eastward across the ocean to Africa. In the Indian Ocean, observe how the elliptical current keeps on the south side of the equator, and avoids the islands and the coast of southern Asia. It is plain then that this hypothesis, when brought face to face with facts, is entirely discredited.

Taking for my guidance the most authentic maps and charts that I could obtain, I have found that an elliptical current does not in any instance extend much farther from the equator than the forty-fifth parallel. I do not think that in any case it reaches as far as the fiftieth parallel, though, of course, the change of the seasons produces some variations. In all five of the oceans, very near the forty-fifth parallel, the current flows due east nearly across the ocean to which the current belongs.

I find, also, that in each ocean, on the western side, there is a point where the current changes its course from westerly and begins to move easterly, and in all cases this point is very near the twenty-fifth parallel. On the eastern side of the ocean there is an analogous turning point, but it is near the thirty-fifth parallel, that is, it is ten degrees farther from the equator than the turning point on the western side of the ocean. These turning points, at some seasons, vary somewhat from the parallels I have assigned to them, but in every case it will be found that the eastern turning point is about ten degrees north of the western.

A true theory should explain these points and limitations of the currents, and give good reasons for every one of their peculiarities.

ELLIPTICAL CURRENTS.

Let us take the North Atlantic current as an example by means of which to illustrate the laws that govern all elliptical currents. This current may, for convenience, be said to commence in the Gulf of Mexico, at the twenty-fifth degree of north latitude. Here the water of the current is neutral, that is, it has the same easterly velocity that the globe in the same latitude has. From the Gulf the current flows north-east. It flows north because it is warm, and east because it carries with it more easterly force or easting than is possessed by the globe in more northern latitudes. The farther north it flows the more easting it acquires, and the more it differs from the places in which it arrives. When it reaches the forty-fifth degree of north latitude at the Grand Banks of Newfoundland it has acquired so much easting that it flows due east nearly across the ocean.

This will not appear incredible when we consider that the transferred water left the Gulf with an easterly velocity of two hundred and five miles per hour greater than is possessed by the water on the Grand Banks. The wonder might rather be that the current did not flow due east before it reached as far north as the Grand Banks.

The question may occur why the water of the ocean current only moves about two miles an hour. If the water of the Gulf could by some means be instantly transferred from the twenty-fifth to the forty-fifth parallel, it would move eastward two hundred and five miles per hour. The reason why the current only moves two miles instead of two hundred is that the quantity of the water transferred from the Gulf to the Banks is small compared with the great mass of ocean, and the force of a particle of the transferred water is diffused and divided among a great many particles, so that the current gains in mass what it loses in velocity.

Many authors, finding it necessary to give some reason for the current turning east so suddenly at the Grand Banks, assert that it is deflected east by the Banks themselves. But when we consider the great difference in easterly velocity (205 miles per hour), between the twenty-fifth and forty-fifth parallels, we need be at no loss for the reason why the current turns from north-east to due east.

Before the current reaches the shores of Europe it turns to the south of east; this also has been vaguely accounted for by saying that it is deflected by the shores. But the truth is, that it has become cooled in crossing the ocean. The warm water at the surface cooled first and sank and turned southerly; its place was immediately occupied by the warmer particles below, and these in turn cooled, sunk and turned south in the same manner. So much of the current as did not thus become cooled turned to the north-east, and flowed to the Arctic ocean; but the cooled portion flowed south, as all cold ocean water in the northern hemisphere does.

At the thirty-fifth parallel the current ceases to flow easterly, and turns to the south-west. The reason of this change is that the surplus easterly force is exhausted. Here, therefore, the water becomes neutral. It has just the same easterly velocity that the globe in that latitude has, and no more. Let me here remark, that in each of the five great ellipses there are two neutral points where the current and the globe have the same easterly velocity; one of these is at the twenty-fifth and the other at the thirty-fifth parallel. I doubt whether either of these points ever varies five degrees from those parallels.

It will be observed that the current begins to flow easterly at the twenty-fifth parallel and that it does not reach the same parallel again before it ceases to move easterly. It falls short ten degrees, and ceases to flow easterly at the thirty-fifth parallel. If the current could move, as a planet does, without expending any of its force, it would reach the twenty-fifth parallel before its easterly force would be exhausted, but the current expends so much of the easterly force that it carries with it from the twenty-fifth parallel, that it is exhausted when it arrives at the thirty-fifth parallel. It seems, therefore, that the easterly force expended by the current on its journey from the twenty-fifth parallel to the thirty-fifth, is equal to the force required to continue it in an easterly direction ten degrees farther.

When the cold neutral current flows south of the thirty-fifth parallel, it immediately enters latitudes that possess more and more easterly velocity than the current does. Of course the current flows relatively south-west. The farther south the current flows, the more westerly it flows, until, near the equator, it is impelled in a due west direction by an irresistible force.

I have already explained that when the current flows due east, it continually cools, and turns southerly; but now, while flowing due west near the equator, it becomes warmer and warmer, and the heated surface water turns northward, as all warm current do in the northern hemisphere. The current, therefore, flows north-west to the Gulf of Mexico, whence it started. Here its westerly force is exhausted, it becomes neutral again, and is ready to repeat its elliptical circuit.

It is an interesting, as well as important fact, that the laws which govern the currents are such as to limit the extent of the ellipses, and fix their boundaries in all directions.

In the Atlantic the ellipses have not quite room enough to develop themselves to their full extent, but in the Pacific there is evidently "room and verge enough" to enable them to expand, and exhibit their proper normal dimensions. At the first thought it might seem, that, if North America were to sink, the Atlantic and Pacific being one great ocean, there would be but one vast elliptical current, where now there are two; but a more rigid examination will result in the conviction that the two currents would still continue to flow in separate orbits, and perfectly independent of each other. Let us once more briefly review the ellipse and show that the path of a current is fixed by laws as definite and inexorable as those that determine the paths of the planets in their orbits.

When a current flows north-east from the twenty-fifth parallel, it continues to accumulate more and more easting, until it reaches the forty-fifth parallel, when its easterly force is so great that it is compelled to flow due east. The extent of the ellipse in a northern direction, is therefore limited to the forty-fifth degree.

When the current reaches the thirty-fifth parallel, the easting is exhausted, and therefor the farther extension of the ellipse in that direction is impossible. When the current flows near to the equator it has accumulated so much westing that it is forced to flow due west; it cannot cross the equator, and the extent of the ellipse is thus limited. When the current flows north-west from the equator it reaches the twenty-fifth parallel, and then the westing being exhausted the ellipse cannot be extended any farther west. It seems that a current cannot flow alternately north and south between the equater and the forty-fifth parallel without assuming an elliptical or rather an ellipsoidal both essentially like that in the diagram.

In regard to Local currents, they never flow within the limits of an elliptical circuit, but are generated outside of them by excessive cold or heat in situations where the water cannot restore its equilibrium by means of any one of the great elliptical currents.

It may be said that all currents tend to flow in elliptical circuits, but local currents are so situated that they have not room enough to make a circuit and return. They therefore flow in curvilinear paths until they are lost and merged in the great ocean. On the *eastern* side of any ocean a *warm* current is always local and flows counter to the course of the elliptical current. On the *western* side a *cold* current is always local and flows counter to the course of the elliptical current. Our local current never flows counter to another in the same latitude and on the same side of the ocean.

The most remarkable of all the local currents, if indeed it can be properly called local, is the Atlantico-Arctic current.

It flows north-east from the middle of the Atlantic in the forty-fifth degree of north latitude, and passing through the British Archipelago, it flows along the coast of Norway into the Arctic ocean. By this time it has acquired so much easting that it flows nearly due east half way around the pole, and then escapes along the east coast of Greenland into the Atlantic. Acquiring more and more westing as it flows more southerly, it hugs the United States coast and proceeds as a deep cold current into the Caribbean Sea. Its *tendency* now is to escape west into the Pacific, but the American continent prevents this and it remains until it becomes warm, and then turns and retraces its course north-easterly in company with the regular elliptical current, until it reaches the forty-fifth degree, and then repeats its circuit.

The most noted local current in the south Atlantic is called the Guinea current. It appears to be generated near the equator, between the two ellipses, and flows south and south-east into the Gulf of Guinea, between the elliptical current and the African coast. It flows parallel with the elliptical current but in the opposite direction. Malte-Brun say that "no adequate cause can be given for this current." I can readily understand that in the absence of the principles explained in this essay, not only Malte-Brun but every other geographer would be unable to assign an adequate cause for two large currents flowing side by side, but in opposite directions.

The Cape Horn current in the North Pacific, is perfectly analogous. It is a warm current generated on the West Coast of South America, which of course flows south and east along the coast and escapes into the Atlantic around Cape Horn. It is described, I believe, by all writers, as a part or branch of the great cold current (the elliptical current) that flows from the Antarctic ocean to Peru. But the truth is that it is a perfectly independent warm current, flowing south and east, while the great Peruvian current is cold and flows north and west. In the North Pacific, near the equator and between the two great ellipses, is a counter local warm current which flows east to the American coast, and then north between the coast and the ellipse, as far as Oregon, and perhaps farther. I have been informed by Commodore Hitchcock of the American Navy, that he has observed a similar current in a corresponding situation in the Atlantic. I presume that the current which he observed is a part of the so called Guinea current. I regard the Guinea current, the Cape Horn current, and the North Pacific counter current, as all analogous to each other.

There are some so called periodical currents in the land-locked seas of Arabia, Bengal, China and Australia, which are supposed to be produced by the monsoon, or season winds. But there is a lack of correct information concerning them. I suspect, however, that the winds have but little agency in producing these or any other important and deep currents. It is more likely that the same changes of the season which produce the monsoon, or season winds, are also the real causes of the periodical, or season currents of the sea.

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3. THE NATURE OF ELECTRIC DISCHARGE. By Prof. O. N. STODDARD, of Oxford, Ohio.

THAT theory seems very unwieldy which conceives of electricity as two fluids imponderable and indefinitely rare; quiet when united but intensely active when separated, and that all the violent effects are due to the transfer of the fluids through bodies. And this conception constantly manifests itself in the statements of the laws and phenomena of electricity.

Few, it is believed, will object at present to the view that the electric force is molecular; that whatever doubt may exist as to its precise nature and mode of operation, that it is connected with and acts by and through those particles which chemistry contemplates as atomic; that whatever changes or disruptions are wrought by it, are operations going on among these atoms; and that electric discharge is not the transfer of matter, but of some change along the line of discharge, producing other correlated forces. All electrical excitement preceding discharge is a state of induction, transferred, according to Faraday, along lines of particles to other bodies. This condition is one of tension, and the tendency of the particles is to undergo some change antagonistic to cohesion, and consequently resisted by this force. As long as cohesion can resist the electric force, the condition is statical. If the tension rises to a sufficient degree, the particles yield and undergo the change which the force requires. This is discharge. The change, in which consists discharge, will be violent in proportion to the degree of tension, and the character of the change will depend upon the nature of the body. The substance may be split—torn asunder—as in trees; ground to powder or fractured, as in glass and other brittle bodies; melted as in metals, or chemically separated as in compound bodies. These various actions are the equivalents of the electric force, and are its correlated forms.

The energy, in that particular form we call electricity, has in each of these cases of discharge been expended. As a cause it has acted, and as such has disappeared. Its farther existence must be sought for in its effects. The mechanical and chemical separations, the heat and the light, are all so many evidences of the electric force, and exact measures of it.

That all these changes are the results of molecular displacements seems plain from the effects. When passed through a thick plate of glass the discharge pulverizes along its track the glass to an impalpable powder. A violent separation of particle from particle has taken place, as if the molecular interstices were filled with an explosive substance.

A blow from a hard body, or from a fluid shot with enormous velocity, against the glass, would merely drive the particles before it and leave a clear opening. But in electric discharges each particle is so disrupted from the adjacent ones as to break up the cohesion, while none are thrown off except at the surface, where there is no force to counteract the explosive action. The pulverized atoms must occupy more space on account of diminished density, and naturally press outwards in the direction of least resistance. The burr on both sides of a card merely indicates the strong molecular repulsion acting from within outward. The imprisoned air in bodies of loose texture will have its influence, being itself subject to the same disruption, but the double burr will be produced in thin plates of wax, in which air cannot be present in sensible quantity. When a tree is riven by lightning the splintering is caused by the repulsive energy acting along the line of discharge. If the fibres of the wood, where the fracture takes place, had been changed to gunpowder, or gun-cotton, and exploded, the results would closely represent those effected by electricity.

So when a body is melted by the discharge, the heat in producing liquidity certainly acts upon the molecules. But it is unreasonable to suppose that the heat is developed by the discharge merely on the surface, and then passed inward by the slow process of conduction. On the contrary, it flashes into intense energy, at the same instant, among all the particles of the mass. The inconceivable rapidity of the change from a solid to a liquid, caused by electricity, leaves no time for conduction.

That chemical affinity is a molecular force no one, it is presumed, will doubt. But the very intimate relations of the chemical and electric forces — so intimate as to induce in some minds a belief of their identity — will hardly permit us to assign a molecular character to one and not to the other. The light which pervades the atmosphere for some distance around the point where lightning strikes the earth, does not seem, from the cases the writer has had the dangerous pleasure of witnessing, to be a gleam of reflection from the brighter track of the spark, but rather a light produced in the air, for some distance around, by molecular disturbance; the same in kind, but less in degree, as that which rivals sunlight along the central path. A green tree, at which the writer happened to be looking the moment it was struck, was covered from top to bottom with a diffused flame, in the midst of which the central stream of fire gleamed in tortuous course along the trunk. It is not to be supposed, however, that the electric induction, and the tension caused by it, which precedes discharge, exist only along the line of violent action.

The line of discharge may be considered the *resultant* of the electric tension for some distance around, and along this line is expended at the moment, all the accumulated force from inductive action. The violently disruptive effects which sometimes occur, are adequately explained by the heat produced along the line of discharge.

No other known agency generates heat so suddenly and of such intensity as the electric force. The reason is plain. The elctric force being itself molecular, developes, or as some would prefer, is *converted* into heat at the same instant throughout the whole body, and its repulsive power is exerted simultaneously between all the particles, and needs no transfer by conduction. The violence with which some chemical compounds explode, is proportional, other things being equal, to the consentaneousness of the action among the particles. Allowing the velocity of electricity to be the same as that of light, the time in which a coin or metallic button on the person, would be melted is inconceivably short, less than the one *twelve billionth* of a *second.* The same explosive action expended on the air causes the terrific sound of thunder. Near the path of discharge, it is a crash, sharp, spiteful. At a distance it is toned down by the elasticity of the air into a roar which shakes the very earth.

It seems strange that some writers should attribute the sound to the *collapse* of the air after displacement. The collapse is but the reaction from the condensed air around the line of displacement, and of course cannot exceed the force of displacement. The latter, then, is the primary cause of the sound; the collapse only repeats the undulation with some loss of force.

Surprise is sometimes expressed that the human body when struck is not torn.. The electricity is a good deal diffused through so large and so good a conductor, but the texture of the body is of such a nature—composed of porous and elastic solids filled with liquids—that its particles readily yield to the requirements of the force, without disruption.

The mechanical and perhaps chemical disturbances of the nervous system—even if there be no other—are quite sufficient to account for the fatal effects of the electric shock. If it is allowable in a scientific article to introduce one's own experience of an electric shock more violent than one usually takes, then the writer would say, that the effects of a discharge from a battery of twelve jars, one gallon each, passed through the length of the body, is best expressed by the simple phrase —*a stunning blow.*

If the general laws of electric discharge, which I have stated, are correct, then our precautions for safety should conform to them. A lightning rod is intended to afford a ready passage between the cloud and earth, to any electricity which may be accumulating. If it discharges the electricity as fast as it is generated, then there can be no collection in quantity or intensity sufficient to cause violence.

The rod is a good conductor; that is, its particles readily assume the conditions required for discharge; hence it offers a comparatively unobstructed channel for the passage of the electricity, and of course prevents any dangerous accumulation. The rod takes time by the forelock—it anticipates the danger. It does not allow this fierce agent to *store up* its power, but gradually and safely transmits to earth an amount of electric energy, which, if discharged at once, would have manifested intense violence. If the powder in a cannon were burned grain by grain, during a few seconds, it would not even move the ball; and yet the same force has been expended, as, when exploded at once, it hurls the missile with deadly force. Of late I have been inclined to consider the electricity of the cloud to bear to that of the earth the relation of *intensity* to *quantity*. That an equivalency of force may exist between intensity and quantity is acknowledged by all. Quantity in the galvanic battery has its equivalent force in the intense flash of the induction coil. It may also be admitted that quantity in the earth is represented by intensity on the points of the rod. In this case an identity of action is established between the points and the cloud. It hardly need be said that to insulate a rod from the building it is intended to protect is useless, if not worse. The *house* is in a state of electric tension as well as the rod, and the object of the rod is to relieve all such tension. To do this the rod must be in electric communication with the house. Indeed the insulation is after all a sheer pretense, and is practically impossible; for the building and rod *are* connected through *the earth*. If the roof is metallic all the better. It affords surface to diffuse the force, sharp edges and innumerable points to discharge it. Erect the rods then upon the roof.

No better conductors can be constructed to connect such a roof with the earth than its water pipes. Pass iron or copper rods from these pipes into moist earth; or better still, connect one or more of them with the water-pipes of a city, or with the water of a well; or in lieu of these, with the gas pipes, and insurance of such buildings against damage would be profitable to a company at exceedingly low rates.

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4. INFLUENCE OF THE MOON UPON THE WEATHER. By Prof. ELIAS LOOMIS, of Yale College.

SEVERAL meteorologists have attempted, by a comparison of a long series of observations, to determine whether the Moon exerts any influence upon the weather. From a comparison of twenty eight years of observations in Germany, Schübler, in 1830, deduced a sensible influence of the moon, the number of rainy days at the time of the second octant being 25 per cent. greater than at the time of the fourth octant. From a comparison of observations made at Paris, Orange and Carlsruhe, Gasparin arrived at results not differing greatly from those of Schübler.

By a comparison of sixteen years of observations at Greenwich, nine years at Oxford, and sixteen years at Berlin, Mr. Harrison, of England (Ast. Soc. Month. Not., v. 28, p. 39), has obtained results which are remarkably consistent with each other, and which indicate that the moon exerts an appreciable influence upon terrestrial temperature, the maximum occurring six or seven days after new moon, and the minimum about four days after full. The difference between the maximum near the first quarter, and the minimum near the last quarter, is two and a half degrees of Fahrenheit. These results which are so different from what might have been anticipated, Mr. Harrison explains by supposing that the moon really attains its greatest heat about the last quarter; but that the heat which the moon radiates to the earth is entirely dark heat, and therefore absorbed by our atmosphere. This heat raises the temperature of the air above the clouds, causing increased evaporation from their surface, by which they are dispersed; and thus there is an increased radiation of terrestrial heat to the sky, and consequently a diminution in the temperature of the air near the ground. He supposes that opposite results must occur at the period of minimum heat in the moon.

Upon extending the comparison to forty three years of observations at Greenwich (Rep. Br. Asso., 1859, p. 193), Mr. Harrison still finds a fluctuation of temperature, but the range is reduced to 1°.1 instead of 2°.5.

Buys Ballot on tabulating a series of seventy years mean daily temperatures, according to the moon's age, found that the highest temperature occurred during the seven days after full moon; being almost precisely opposite to the results of Mr. Harrison.

Schiaparelli (Mem. del R. Ist Lomb., v. x,) has made a careful analysis of thirty eight years of observations made at Vigivano, near Milan in Northern Italy, and has obtained results which are also remarkably consistent with each other. They show that about the time of the last quarter of the moon, there is a maximum in the number of rainy days, as also in the frequency of storms and in the degree of cloudiness.

At the meeting of the American Association in 1853, I presented the results of seven years observations of the amount of cloudiness at Greenwich. The average results for each day of the moon's age ranged from 6.1 to 7.0; but the fluctuations did not follow any obvious law. If, however, we eliminate the influence of accidental causes, by substituting for each day's result the average of five days (including the two preceding and two following days), the numbers thus obtained are remarkably consistent with each other, as shown in the following table, where the degree of cloudiness is expressed in tenths of the entire sky.

AVERAGE CLOUDINESS.		AVERAGE CLOUDINESS.		AVERAGE CLOUDINESS.		AVERAGE CLOUDINESS.	
3 days bef. new,	6.48	3 days bef. 1st qr.,	6.58	3 days bef. full,	6.78	3 days bef. 3d qr.,	6 76
2 " " "	6.54	2 " " "	6.60	2 " " "	6.74	2 " " "	6.92
1 day " "	6.60	1 day " "	6.60	1 day " "	6.76	1 day " "	6.86
New moon,	6.60	First quarter,	6.64	Full moon,	6.68	Third quarter,	6.76
1 day after new,	6.60	1 day after 1st qr.,	6.66	1 day after full,	6.70	1 day after 3d qr.,	6.64
2 days " "	6.68	2 days " "	6.68	2 days " "	6.70	2 days " "	6.58
3 " " "	6.62	3 " " "	6.70	3 " " "	6.72	3 " " "	6.52

The general character of these results will be best perceived from the curve line, fig. 4, on Plate I. Above this curve I have placed the curve, fig. 3, showing the number of rainy days for each day of the moon, according to the results of Schiaparelli. The general agreement of the two curves is quite remarkable, and seems to indicate a real influence of the moon upon the clouds, the cloudiness being greatest about the time of last quarter.

In order to compare the influence of the moon with that of the sun, I have taken the average cloudiness at Greenwich as indicated by observations made every two hours for a period of seven years. The following table shows the result, the hours being expressed in Göttingen mean time.

DATE.	CLOUDINESS.	DATE.	CLOUDINESS.	DATE.	CLOUDINESS.	DATE.	CLOUDINESS.
0 h	7.14	6 h	6.67	12 h	6.20	18 h	6.90
2	7.14	8	6.29	14	6.49	20	6.97
4	7.03	10	6.06	16	6.80	22	7.14

The difference between the greatest and least cloudiness is 1.06, which may be regarded as a measure of the sun's influence. The moon's influence, as shown in the preceding table, is 0.44, indicating that the sun's influence is two and a half times that of the moon.

In order to determine whether the moon exerts any appreciable influence upon terrestrial temperature, I have arranged the observations made at Girard College from 1840 to 1845, according to the phases of the moon, and taken the average for each day of the moon's age, as shown upon the accompanying sheet. If now, in order to eliminate the influence of accidental causes, we substitute for each day's result the average of five days (including the two preceding and two following days), the numbers thus obtained exhibit a remarkable consistency, as shown in the following table.

AVERAGE TEMPERATURE.		AVERAGE TEMPERATURE.		AVERAGE TEMPERATURE.		AVERAGE TEMPERATURE.	
3 days bef. new,	52°.83	3 days bef. 1st qr.	53.12	3 days bef. full,	52.25	3 days bef. 3d qr.	52.62
2 " " "	52.74	2 " " "	53.31	2 " " "	52.36	2 " " "	52.46
1 day " "	52.75	1 day " "	53.68	1 day " "	52.58	1 day " "	52.63
New moon,	52.56	First quarter,	53.48	Full moon,	52.82	Third quarter,	52.89
1 day after new,	52.46	1 day aft. 1st qr.	53.06	1 day after full,	52.82	1 day after 3d qr.	52.97
2 days " "	52.76	2 days " "	52.71	2 days " "	52.70	2 days " "	53.07
3 " " "	52.97	3 " " "	52.67	3 " " "	52.64	3 " " "	53.14

The general character of these results will be best perceived from the curve line, fig. 2, on Plate I. The range of the fluctuations is considerably reduced by taking the mean of five

Date
1840
1841.
1842.
1843.
1844.
1845.
Means.

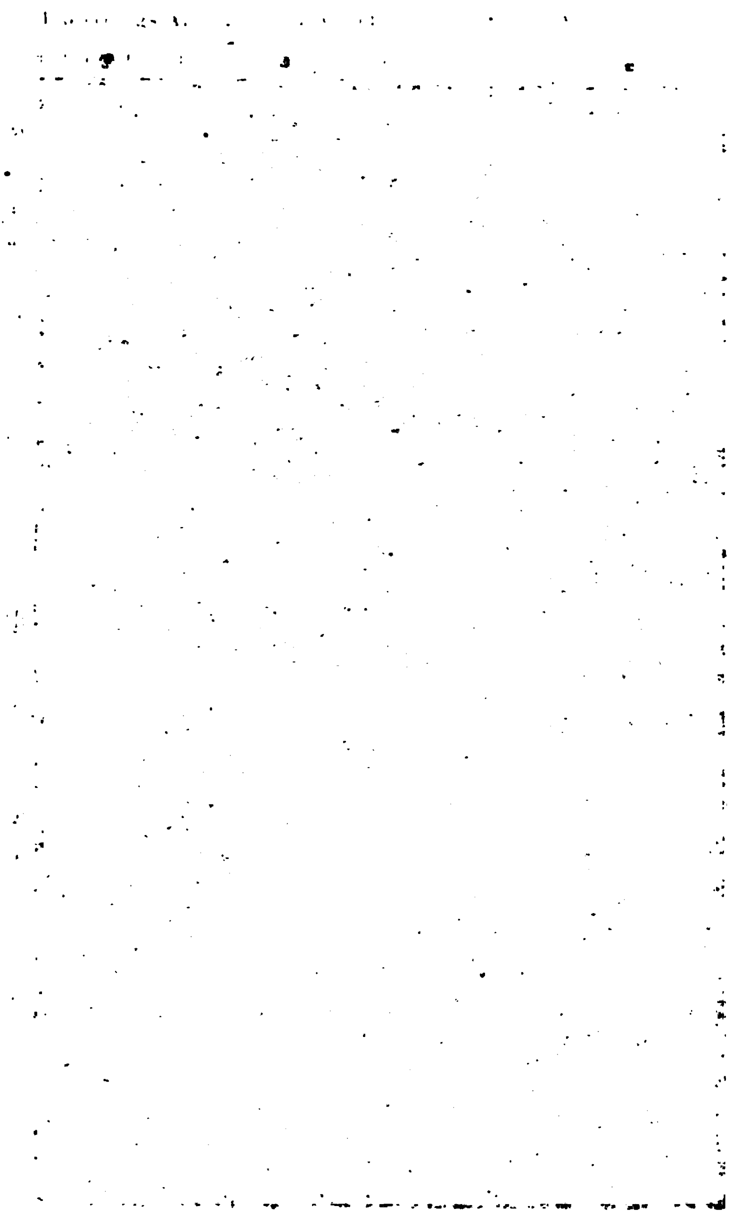
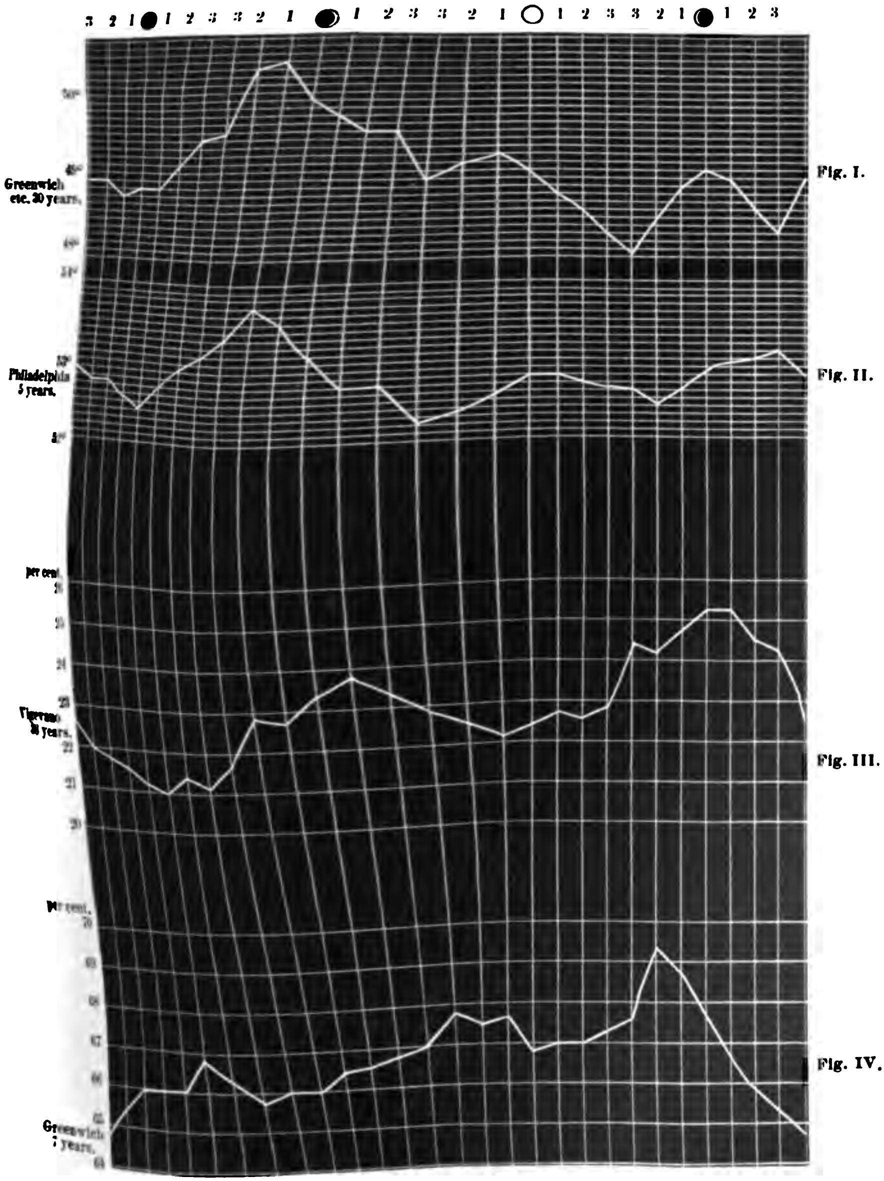


Fig. III

Fig. IV.

MEAN TEMPERATURE AND CLOUDINESS ACCORDING TO THE AGE OF THE MOON



CURVES OF MEAN TEMPERATURE AND CLOUDINESS ACCORDING TO THE AGE OF THE MOON.

successive days; but the characteristic features of the curve are still preserved. Above this curve I have placed curve line, fig. 1, representing the mean result of thirty years' observations at Greenwich and Berlin. It will be perceived that both series of observations indicate a maximum temperature a day or two before the first quarter, and the principal undulations of the two curves exhibit a similarity so decided as to afford a strong presumption that they are mainly due to lunar influence. The explanation assigned by Mr. Harrison is, however, contradicted by the observations represented in fig. 4, which shows that the cloudiness of the sky is greatest about the time when the heat of the moon should be the greatest.

The mean diurnal change of temperature at Greenwich and Berlin is a little over ten degrees, which represents the average diurnal effect of the sun's rays at these places. The monthly fluctuation of temperature, which is apparently due to the moon's influence, is somewhat over two degrees; or the influence of the sun is about five times that of the moon. Nearly the same ratio results from the observations at Philadelphia.

In 1853, Althaus (Pog. An., v. 166, p. 544) undertook, by improved methods, to measure exactly the amount of the solar radiation; and he applied his method to determine the effect produced upon the moon's surface. His conclusions were that the temperature of the moon's surface fluctuates from considerably below zero to several hundred degrees above zero, and attains its maximum about seven days after full. This last conclusion coincides with the results deduced by Buys Ballot from seventy years' observations in Holland (Pog. An., v. 146, p. 163), but seems at variance with the observations made at Greenwich and Philadelphia.

The effect of the moon upon the cloudiness of the sky appears to be decided, and to follow nearly the same law as the influence of the sun. The mean degree of cloudiness is greatest about noon, and least about nine o'clock in the evening; that is, the maximum of cloudiness slightly precedes the hour of greatest heat, and the minimum follows the maximum at an interval about equal to one third the time of the sun's revolution. So, also, the greatest cloudiness due to the moon's influence occurs about the time of greatest heat; that is, near the third quarter; and the minimum follows about a week after the maximum, or about one fourth the time of the moon's revolution.

This disturbance of the vapor of our atmosphere gives rise to storms, during which vapor is precipitated, and a vast amount of latent heat is liberated. The motion of these storms over the earth's surface is influenced by local causes, such as geographical latitude, proximity to mountains or the ocean, and hence the effect of these storms upon terrestrial temperatures may not be everywhere the same. This principle may, perhaps, explain the apparently opposite effects of the moon upon the temperature of different localities.

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5. On the Relative Value of Gold and Silver for a series of years.* By E. B. Elliott, of Washington, D. C.

(ABSTRACT.)

The speaker began with an account of the relative standard value of gold and silver adopted by different nations in their coinage. His object was a practical one, bearing on the recommendations of the commission on a uniform system of coinage lately in session in Paris, in connection with the Exposition. Formerly the French adopted 1 to 15½ for the ratio of the value of silver to gold in their coinage. After the influx of gold from California and Australia, the market value of gold relative to silver fell below this ratio, and the inevitable effect was that the silver coin was remelted or exported nearly as fast as coined.

To obviate this evil, the silver coins smaller than the five-franc piece were debased, in accordance with a recommendation of a quadripartite convention concluded in 1865, between France, Belgium, Italy and Switzerland. This debasement Mr. E. conceives to be a mistake. They ought instead to have increased the amount of gold in the gold coinage about 3⅓ per cent., thereby conforming to the simpler, and in every respect preferable ratio of 1 to 15. There would then have been no

*This paper was read at Burlington, and was accidentally omitted in the printed volume of Burlington Proceedings.

need of changing the fineness of their smaller silver coinage from the simple decimal ratio, nine-tenths which had previously been declared "immutable," to the more complicated one of 835 thousandths. Then the new gold five-franc piece would have weighed one-third of five grammes, a very simple ratio, and would have been *almost the exact value of the American gold dollar.* The system of the United States is almost strictly metrical. The new French system (quadripartite convention) is unmetrical, having complicated relations to their beautiful system of weights and measures. Yet the international commission has recommended the adoption of the latter, and thereby the perpetuation of what originally was simply a mistake. Whereas, had a gold coin less only by one third of one per cent. than the American gold dollar been adopted as the standard, the French silver coinage could be maintained at its old standard fineness of nine tenths, and would continue in entire harmony not only with the metric system, but also with the present and past market values of the metal; while the American coinage would not require appreciable change.

The paper was illustrated by a diagram.

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6. A NEW FORMULA FOR THE REDUCTION OF OBSERVATIONS IN THE PRIME VERTICAL, ANALOGOUS TO MAYER'S FORMULA FOR THE REDUCTION OF MERIDIAN OBSERVATIONS. By WILLIAM A. ROGERS, of Alfred Center, N. Y.

IT is easily proved:—

I. That, if from any point of the celestial sphere as a pole, great circles be drawn through two points nearly equidistant from the assumed pole, then, for any small arc, the distance between the two points will be equal to the angle at the pole multiplied by the sine of the polar distance.

II. That the time required to describe a small diurnal arc near the meridian, is equal to the given arc divided by the cosine of its declination.

III. That the time required to describe a small diurnal arc near the prime vertical is equal to the given arc divided by the product of the sines of the latitude and the zenith distance.

Let, *a*. *b*. *c*.

equal the azimuth, level and collimation constants. Then, by I, for any zenith distance these functions become:—

$a \sin n$. $b \cos n$. c.

For either the meridian or the prime vertical,

The time of transit over the given threads = the observed time.
+ the clock error.
+ the time of describing the arc $a \sin \phi$.
+ the time of describing the arc $b \cos \phi$.
+ the time of describing the arc c.

For the meridian, by II,

$$T = T' + \Delta T' + \frac{a \sin z}{\cos \delta} + \frac{b \cos z}{\cos \delta} + \frac{c}{\cos \delta}$$

which is Mayer's formula.

For the prime vertical, by III,

$$T = T' + \Delta T' + \frac{a}{\sin \phi} + \frac{b}{\sin \phi \tan z} + \frac{c}{\sin \phi \sin z}$$

which is the form given by Buïnoso.

But this equation is true only for a single thread, whose distance from the line of sight is a constant quantity c. Since, during the passage of a star over several threads, the zenith distance undergoes a continual change of value, the mean of the observed times on the several threads will not coincide with the time on the mean of the threads. In order, therefore, to apply the preceding formula, it is necessary to reduce the time on each thread to the time on the mean of the threads by the usual formula.

$$I = \frac{i}{\sin \phi \cos \delta \sin (\theta' - T)}$$

an operation which is exceedingly tedious, and likewise requires a knowledge of the thread intervals.

I have found that the reduction from the mean of the observed times to the mean of the threads, can be completely effected by the simple expression,

$$\theta - \theta' = [.0031681^s] \text{ [no. units in log. k] } \cot \theta'$$

where,

θ = the hour angle for the mean of the threads.
θ' = the hour angle for the mean of the observed times.
log. k = a function of the variable hour angle, and is to be taken from Table VIII, Chauvenet's Astronomy, Vol. II.

The formula for the mean of the threads therefore becomes:

$$T = T' + \Delta T' + [.0031681^s]\ [\text{no. units in log. k}]$$

$$\theta + \frac{a}{\sin\phi} + \frac{b}{\sin\phi \tan z} + \frac{c}{\sin\phi \sin z}$$

The following reduction is given as a test of the accuracy of the formula and also to show its superiority over the ordinary form both in brevity and simplicity.

By the ordinary form:—

CONSTANTS.

$\theta' = 4°\ 38'\ 24''$ log. sin ϕ cos δ = 969,761.

T′ I ½I $\theta-\frac{1}{2}$I log. sin ($\theta-\frac{1}{2}$I) log. sin ϕ cos δ sin ($\theta-\frac{1}{2}$I) log. i log. I I T.

Thr'ds.	T′ h. m. s.	I m. s.	½I ′ ″	$\theta-\frac{1}{2}$I ° ′ ″	log. sin ($\theta-\frac{1}{2}$I)	log. sin ϕ cos δ sin ($\theta-\frac{1}{2}$I)	log. i	log. I	I m. s.	T ° ′ ″
1	9 50 51.0+5	22.8+40	21—5	13 45	8.95971 n	8.65782 n	1.18033 n	2.47310+4	57.2	9 55 48.2
2	9 51 52.0+4	21.8+32	43—5	6 7	8.94904 n	8.64665 n	1.02825 n	2.37660+3	58.0	50.0
3	9 53 54.2+3	19.6+24	57—4	58 21	8.93791 n	8.63552 n	.87564 n	2.24012+2	53.8	48.0
4	9 55 15.2+0	58.6+ 7	19—4	40 43	8.91151 n	8.60912 n	.11727 n	1.50815+0	32.2	47.9
5	9 56 29.2—0	15.4— 1	55—4	31 29	8.89702 n	8.59463 n	.19612	.59849—0 n	39.7	49.5
6	9 59 9.2—2	55.4—21	55—4	11 29	8.86385 n	8.56146 n	.87040	2.30894—3 n	23.7	45.5
7	10 0 48.2—4	34.4—34	18—3	59 06	8.84195 n	8.53956 n	1.01995	2.48039—5 n	2.3	45.9
8	10 2 31.6—6	17.8—47	15—3	46 11	8.81787 n	8.51548 n	1.12743	2.61195—6 n	49.2	42.2

	h. m. s.
The mean value of T′ =	9 56 13.8
The mean value of T =	9 55 47.2
Difference =	26.6

By the formula,

$$\theta - \theta' = [.0031681^s][\text{no. units in log. k}] \cot \theta'$$

log. .0031681 = 7.50081

log. 668 = log. no. units in log. k = 2.82478

log. cot θ' = 1.09856n

log. $(\theta-\theta')$ = 1.42415n

$\theta-\theta'$ = —26.6s

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7. On the relation between the Atomic Volume of different metals, and their Paramagnetic and Diamagnetic Properties. By P. H. Vanderweyde, of New York.

When we divide the specific gravity of an elementary substance into its atomic weight, we obtain a quotient which has been called the Atomic Volume. On the supposition that the so called atomic weights represent really the relative weights of the atoms, these quotients would indicate the relative distances of these atoms. It has been observed that these different quotients lay close together, so that a group of some substances will give a quotient of which the mean is about $3\frac{3}{4}$, others $4\frac{1}{2}$, others 10, and others 20, etc.; but as far as I have been able to ascertain it has never been observed that there exists a most remarkable relation between these quotients and the magnetic properties of the different metals, as the following table shows:

NAME OF METALS.	SPEC. WEIGHT.	ATOMIC WEIGHT.	QUOTIENT.	REMARKS.
Cobalt.	8.5	30	3.53	Paramagnetic at bright white heat.
Iron.	7.8	28	3.59	" only below bright red heat.
Chromium.	7.	26.2	3.74	" " " dark red heat.
Nickel.	8.8	29	3.3	" " " 600° F.
Manganese.	7.1	27	3.80	" " " 4° F.
Palladium.	11.3	53	4.49 }	Paramagnetic with strong electro-magnets.
Platinum.	21.5	99	4.57 }	
Osmium.	21.4	100	4.68 }	
Alumium.	2.56	13.6	5.35	
Cadmium.	8.7	56	6.4	
Magnesium.	1.74	12	7	
*Copper.	8.96	63.4	7.07	Paramagnetic with strong electro-magnets.
Lead.	11.445	103.5	9.12	
Gold.	19.3	196	10.005	
Silver.	10.5	108	10.3	
Calcium.	1.58	20	12.6	
*Mercury.	13.59	200	14.36	Strongly diamagnetic.
Tin.	7.29	118	16.17	
Antimony.	6.7	120.3	17.9	Very strongly diamagnetic.
Bismuth.	9.9	208	21.22	Strongest diamagnetic metal after Faraday.
Sodium.	0.97	23	23.7	
Potassium.	0.86	39	45.35	Strongly diamagnetic.

OBSERVATIONS.

1. The five so called magnetic metals give us the smallest quotients laying all between 3 and 4.

2. The commonly so called non-magnetic metals have all quotients above 4.

3. Faraday proved, with powerful electro-magnets, that next to the five so called magnetic metals, Palladium, Platinum and Osmium possess the strongest paramagnetic properties. They have in the above list, also, the next smallest quotient connected with them which lay between 4 and 5.

* I have doubled the atoms of Copper and Mercury, only to reduce the formula of their lowest oxides to that of the other metals MO, in place of M_2O. For the same reason we must, perhaps, double the atoms of Lead also, which would double its quotient.

4. In the same way that diamagnetism is the opposite of paramagnetism, the larger quotients in the preceding table belong to diamagnetic bodies; for instance, Mercury, Antimony and Bismuth. The last is the strongest diamagnetic substance experimented upon by Faraday, and possesses the largest quotient of any of the heavy metals.

5. If this law holds also for the light metals, Sodium and Potassium must be still stronger diamagnetics, which I found in so far verified by experiment, that bars of these metals enclosed in glass tubes were found to be strongly diamagnetic. However, they have not yet been compared with Bismuth.

6. Of the five magnetic metals, the quotients are the smallest for those which are the most permanently magnetic, even at a high temperature and *vice versa*, with the exception of Nickel.

7. As cooling increases, by contraction, the number expressing the specific gravity, it will consequently decrease this quotient, indicating a decrease in the distance of the atoms, in perfect accordance with the fact that cooling increases the paramagnetic properties.

8. If we were able to cool the other metals so as to increase their specific gravity to such a degree as to have a decided effect on the amount of this quotient, we might perhaps succeed in discovering in more of them paramagnetic qualities, by means of the apparatus used by Faraday.

9. As inversely heating decreases, by expansion, the specific gravity, it will increase this quotient, in accordance to the fact that heat diminishes paramagnetism, and finally destroys it in all metals with the single exception of Cobalt, which has a small quotient and, consequently, can stand some increase.

10. Heating decreasing the paramagnetic qualities with the specific weight and the quotient, may do this to such a degree as to make the body first neutral and then paramagnetic. An instance is known where this takes place, namely, oxygen gas: when cool it is paramagnetic like iron, and when hot diamagnetic bismuth.

11. That this relative distance of atoms (upon which, of course, their specific gravity depends,) is closely related to their magnetism, is again proved by the crystals, which, being always less dense in the direction of their optical axis, and

expanding by heat more in one direction than in another, have been proved by Pluecker to be diamagnetic in the direction of their optical axis or longest axis of crystallization. In some of these crystals this action is so strong that they are influenced by the magnetism of the earth; as for instance, a properly cut crystal of kyanite, which will behave like a compass needle when suspended on an axis, a fact also mentioned by Professor Silliman in the last edition of his excellent textbook on Physics.

Attraction, which pervades all nature, may be classified in four grades, the chemical affinity and the cohesion, acting at distances infinitely or unmeasurably small, the very opposite of gravitation, acting at distances infinitely or unmeasurably large. We may also distinguish between chemical affinity and wherein the last may be overcome by mechanical force, which does not affect chemical combination. Then we have finally the magnetic attraction, which we may consider as the cohesive force, otherwise confined within the surfaces limiting the body, manifesting itself outward at measurable distances. I will not occupy the time of the Association in developing this idea, which finds a strong support in the fact that, by proper electric currents, two pieces of iron, for instance, may be made to adhere with a power as if they were one; and I will only remark that the magnetic attraction appears to be the connecting link between the force of cohesion, acting at infinitely small distances, and that of gravitation, acting at infinitely large distances.

We know that heat will counteract either of these attractions, chemical affinity as well as cohesion, as it has been proved that, at a sufficient high temperature, all bodies are not only gaseous, but that also all compounds are separated, and their constituents reduced to their elementary condition; that magnetic attractions are destroyed, and that even gravitation is counteracted, as heat will enlarge enormously the bulk of a celestial body, diffusing its constituent parts otherwise kept together by gravitation. This last idea, or rather fact, gave origin to Laplace's ingenious hypothesis, that the whole universe was once in a nebular heated condition which by cooling contracted. The modern discoveries in regard to the conservation of forces, have modified this hypothesis and at the same time elevated it into a theory, and we hold now that in place of the nebular matter being hot, and concentrated by cooling, it was cold and concentrated by gravitation, coalescing in different centres of attraction, the motion thus produced being at its partial disappearance in these centres, converted into heat. Afterwards this heat radiated out into space, in the form of the sunbeam and the light of the stars, and as it has been proved that the heat and light of our sun is the source of all life and motion on our planet (with the single exception of the tidal wave), which heat of the sunbeam again owes its existence to the primary gravitation which once caused it to attain its high temperature, we may safely state that all life and motion on this planet is nothing but gravitation in disguise.

Gravitation thus, of which the velocity of transmission surpasses that of light, being literally infinite (astronomy proving that its attraction acts instantaneously at any distance), becomes a fundamental law developing itself in a series of consequences of the utmost diversity.

I do not wish to go into speculations concerning the conclusions which may be drawn from the data I bring before you; speculations about the ultimate composition of matter, nature of atoms, the manifestations of their magnetic attractions or repulsions, etc. To an inquiring mind possessing the knowledge of the latest investigations in cognate branches of research, manifold are the speculations to which it gives rise, and I will be happy if others more able than myself, will also give some attention to the subject I have the honor of bringing before this Association.

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8. The Steam Boiler. By J. A. Miller, of New York.

INTRODUCTORY REMARKS.

To give a general idea of the principles I wish to advance, I will represent on the blackboard a flask, partly filled with water, over a gas burner, the mouth being secured with a stopper. As soon as the pressure of steam in the interior is sufficient to force out the stopper, not only is all the steam allowed to escape, but the greater portion of the water is forced out. Now this is a regular boiler explosion, — not very disastrous; yet I have made the same experiment with a similar flask, strong enough to stand a pressure of fifty pounds to the square inch, on which I placed a safety-valve, so arranged that it would lift suddenly at thirty pounds pressure. After placing the same over a fire, an assistant and myself watched the operation through a one-inch hole bored through a board placed before us for safety. A few seconds after the flask had been fifteen minutes on the fire, the safety-valve opened entirely, a powerful jet of steam was forced out of the opening, and instantly after the flask bursted with a loud report, shattering every pane of glass in the windows, and destroying the plastering of the ceiling. The time between the two explosions was infinitely short, and seemed to me just sufficient to force the water against the upper part of the flask, when the whole was shattered into fragments.

If we place into the first flask a cone, and divide the water into a thin film near the heating surface and a larger body of water not exposed to heat; and then place a little powdered amber in the water, we find, on placing the flask on the burner, that the cone of glass is heated first, and thereby heats the water in contact with the same. But a large portion of the heat passes through, or rather is radicated from the inner surface of the flask, which rays being intercepted by the shield, heat the same, and we have a thin film of water in contact with two heating surfaces. This water soon becomes warm and, of a consequence, lighter; it is, therefore, forced upward by the cooler and heavier water in the larger space, hereby producing circulation of the water at first, as will be observed by the particles of amber slowly, yet rapidly, increasing as the heat increases. You will also observe that as the water in the larger space moves but slowly, the amber is gradually deposited in the pocket formed by the shield, and no deposit can collect near or on the heating surface, thereby always, and with any kind of water, keeping the heating surface clean and efficient. Soon the pressure will be sufficient to force out the stopper. Now, what is the result? The stopper is out, and all the water above the shield is hanging in drops on the ceiling. Why not the water below the shield, nor even below the top line of the same? Simply because it contains no steam, but mere solid water; whereas, in the first experiment, there was as much steam, bulk for bulk, in the water as in the steam space.

Now, the third flask is essentially like the first and second, only the shield is carried up to or a little above the water line. When we place it on the gas burner the whole heat is concentrated on the thin film of water between the flask and the shield. The steam is at once carried into the steam space, not by any exertion or force of its own, but by the superior weight of the cooler water. We first get the steam at first cost. Second, we have a rapid absorption of the heat. Third, a perfect guarantee that water is always in contact with the heating surface. Fourth, we have a circulation of water exactly proportioned to the amount of heat given up to the surface, thereby ensuring always, and under all circumstances, a uniform temperature in the boiler plate, no matter what the heat of the fire, and can have no burning out of the same. We have no deposit, but on the contrary, under all circumstances, a bright surface exposed to the fire. And last, we have a perfect, simple and natural reservoir for the collection of any mineral, vegetable or saline deposits, or impurities, contained in the water. Observe well when now the stopper is forced out. I shall hold my hands over the opening, for as no water can be forced out, the steam being on top there is nothing to scald me,—an experiment I should not like to perform in the first instance. The stopper is out, the steam has not scalded my hands, and, as you will easily perceive, the water line is not the least altered.

This, Mr. President and Gentlemen, is the principle I claim, and which I incorporate into all improvements in steam boilers.

The true end, aim, and object of science is the careful investigation of all things that to the mass of mankind are mysterious and unaccountable. Science is never satisfied to merely know a thing *is*, but is ever searching for the cause. Millions of men had seen apples fall before Newton proved to the world the cause; nor is the scientific mind ever satisfied with the simple knowledge of effect produced, but labors unceasingly to discover the laws or causes which produce these effects.

A family in Germany dies after eating ham. They were in ordinary health, and their death could not be explained by ordinary known causes. The facts in the case are hardly stated, when hundreds of scientific men, setting aside all other occupations, concentrate their minds on the investigation of the true cause which produced this effect; and the result is the discovery of a Microscopic Parasite as the cause of death, and also that a temperature of 180° removes this cause, and renders the food harmless.

It is this thirst for positive knowledge that leads men to brave the frosts of the Arctic Ocean, and the burning sands of Africa: it is this that caused Faraday to leave his laboratory and cross two oceans to investigate the Geysers of Iceland; and in this investigation, not only did he discover the laws which govern these periodical discharges of large masses of water, but he showed us one of the main causes of steam boiler explosions.

The steam boiler, as the source of power for the steam engine, has become as necessary to man as the elements. Without it this convention of men devoted to science, from all sections of this vast continent, could not have been held. None could have spared the time, nor endured the fatigue of the journey; nay, even this "Empire City of the West," which has so hospitably received us, and opened to our enjoyment her magnificent hotels, public and private buildings, would not, could not, have existed without its aid.

Invaluable as this power is to man, it would be more so if positively, absolutely safe, which is not the case. Yet who doubts that it can be made so, as safe as a water-wheel?

We have seen how soon science discovered the cause of the death of the German family, and instantly showed a safe and simple remedy. Let the scientific men of our time as carefully and persistently investigate the steam boiler, and the true cause of danger once discovered, the remedy will be at hand. But to arrive at the true cause *nothing* must be left to meritorious conjecture. All actions of heat and water must be carefully investigated, and remedies based on *authenticated facts.*

The ablest document ever presented to the scientific world on this subject, was the Report of a Committee of the Franklin Institute of Philadelphia, by the honored and lamented member of this Association, the late Alexander Dallas Bache. This report has done much to prevent the reckless use of steam boilers, and to it we owe the passage by Congress of the laws for the inspection of steam boilers. Yet, even these laws, stringent as they are, have not reached the whole case. Daily, widows and orphans lament the loss of some dear one sacrificed to this insatiable Moloch. The field is therefore still open, and strict, careful, and patient investigation still a *duty.*

For this purpose I would lay before you the result of investigations carried on during several years, and under the most favorable circumstances, having over 200 steam boilers under personal supervision, boilers constructed under every principle and of every form.

The reliable causes of explosions may be summed up as follows: —

1st. Gradually increasing pressure.

2d. Sudden increase of pressure by exposed highly heated surfaces.

3d. Incrustation.

4th. Sudden contraction and expansion of metal.

The more mysterious causes assigned, such as decomposition, creation of explosive gases, electricity, &c., we will not here consider, since they are purely *speculative.*

Referring to the above well known and *reliable* causes, the first, "Gradually increasing pressure," seems at first sight to be easily disposed of by first testing the strength of the boiler, and then providing a properly proportioned safety-valve. Yet such is not really the case, and a close inspection will distinctly show the reason.

Before us we have a number of drawings of boilers shown in section.*

No. 1, shows a boiler filled with water heated to nearly 212°. When we first apply heat no perceptible change takes

* These diagrams were exhibited at the meeting.

place, except that the temperature is gradually increased, or in other words, the water is gradually heated until the entire mass is at 212° under ordinary atmospheric pressure. When this condition is reached, we may do either one of two opposite things, and produce the same result, viz.: we may add more heat to the water and produce ebullition, or we may relieve the atmospheric pressure by means of the air pump, which produces the same result. Both will expel a portion of the steam contained in the water. Now if a fire is maintained under the boiler, the steam generated accumulates in the steam space and gradually increases the pressure on the water, which pressure in its turn prevents ebullition, until more heat is added; as, therefore, the pressure increases so will the temperature increase likewise.

Now whatever the pressure, ebullition cannot take place until the pressure of steam in the water is in excess of the pressure of the steam in the steam space, for the steam in the water must force its way through a medium 813 times more dense than the steam itself, and subjected to the pressure of the steam in the steam space, nor is even this all; for the steam formed near the boiler iron must expand. When it reaches the water surface it occupies a space some 1700 times greater than it did when it was water. It is self evident, therefore, that a much larger quantity of steam is actually contained in the water itself, than in the steam space. Remembering this fact, let us now open the safety-valve suddenly, and what is the result? a sudden relief of pressure, a violent rising of water under the safety-valve, and a sudden supply of steam from the body of the water. If, now, we have exposed flues, or highly heated metal, with which the steam can come in contact, we will have such a development of force as no steam boiler can withstand, and a sudden rupture of the weakest part of the boiler is the unavoidable result. Now a rupture of any part of the boiler ought to have the same effect as the action of the safety-valve; for the common cry is, "that safety-valves are not of sufficient area to allow the free escape of steam; whereas, a rupture is generally amply large, and yet in truth, the effects are precisely the same. When a boiler bursts, all the water is converted into steam, not purely *dry* steam, but steam mixed with minutely divided particles of water.

The immense force exerted by the explosion of a boiler is looked upon by many as so stupendous that they cannot conceive of the possibility that simple steam can produce such results; and even theories which assume formation of explosive gases, ignited by some mysterious cause, are favorably received.

Anxious to obtain some reliable data regarding the actual power stored in a steam boiler, I made the following experiment.

Selecting four boilers, favorably located and well set, arranged with proper dampers, and well covered so as to retain the heat, I had the steam increased to one hundred pounds per square inch above the atmospheric pressure. Steam was then admitted to a Corliss Steam Engine, which operated six machines, which as it was carefully ascertained, consumed 150 horse-power. After running the engine for one hour the conditions were as follows:

Pressure of steam one hundred pounds, and three gauges of water. The dampers were then carefully closed, the fires drawn, and the engine started. At first a high grade of expansion was used, gradually decreasing until in forty nine minutes the steam followed nearly the whole stroke, or in other words, nearly an entire cylinder full of steam was used during each stroke of the piston. We then stopped two of the machines, working twenty seven minutes; throwing off two more machines we worked them nineteen minutes, when the engine stopped, with a steam pressure in the boiler of twenty three pounds. Water line, between the second and lower gauge. This gives us a power of 150 horses for forty nine minutes, one hundred for twenty seven minutes, and fifty horse-power for nineteen minutes.

When we now consider, first, that in case of an explosion, the whole of this force would have been exerted in an instant, or say one second, we find that the power exerted would have been equal to 660 000 horses, and when we remember the power lost in the engine and that still remaining in the boiler, 1 000-000 horse-power would not be overrating the actual result. So stupendous is this power that the human mind can scarcely conceive its immensity. Now, when such is the power contained in a steam boiler, what need have we for looking for any solution in the mysterious working of the electric fluid, or production of explosive gases? The power stored in the water is much more than sufficient to produce the worst and most disastrous effects.

To illustrate the action of the steam and water when the safety-valve is suddenly opened, I have drawn a line in the section of the boiler in which a simple perforated pipe is shown, which pipe should be used on all boilers; first, as a "dry pipe," and also to distribute the relief of pressure over the whole surface of the water level, for it is well known that when the water has been at rest, the starting of the engine, or the opening of the safety-valve, is the time at which most explosions take place.

Incrustation is the worst enemy of the steam boiler, for in all cases it is a poor conductor of heat, and by preventing the contact of the water with the boiler plates the latter become overheated and consequently rapidly destroyed; particularly as the concave form of our boilers causes the collection of sediment in the lowest point, and as this is the portion of the boiler that is directly over the fire the iron is soon destroyed: entire plates become overheated; the expansion causes the scale to break, when water comes in contact with highly heated, burned and weakened metal; sudden formation of steam is caused, sudden increase of pressure follows, causing rupture, and with it all the disastrous consequences of explosion.

Having thus pointed out the true causes of boiler explosions, the next step is to point out the remedy, and to do this let us return to the water in the boiler.

When we take a vessel containing milk, syrup or any other fluid and place the same over the fire, we find that soon after boiling commences the liquid foams and is in danger of "boiling over," as it is called. If we then stir the liquid large volumes of steam are set free, and boiling instantly ceases. Now what we here do is simply by mechanical means assist the steam contained in the liquid to escape. If we take a vessel in which liquid is violently boiling and pour the contents slowly into another vessel, we produce the same effect by allowing the steam to escape. If we place diaphragms in a boiler we produce a similar effect by creating currents circulating upwards over the edge of the diaphragm, caused by the difference in the specific gravity of "Water filled with steam" and water without steam. Then, if we go still farther, and provide an "eddy" where the water can be comparatively quiet, we shall also provide for the collection of sediment in pocket surrounded by water and not in contact with the fire, as shown in this drawing.

The results are, first, that all steam as soon as generated enters at once into the steam space, and that the sediment being located in a pocket away from the heating surface, the water having at all times free access to the same, making *any* overheating of the plates an *impossibility*. We have in fact, steam, water and sediment, all in their proper positions; we have a bright and efficient heating surface, rapid absorption of the corresponding equal distribution of temperature, and a safe and efficient steam boiler.

Circulating of water in steam boilers has been frequently attempted, and one of these attempts, from which immense results were anticipated, is illustrated in figure 1, of this diagram. This is known as the "Perkins Boiler." It was the cause of an able paper by the late Professor Bache, in which he most conclusively shows, that no matter how desirable the circulation of water in steam boilers is, the Perkins boiler, designed especially to this end, may lay no claim whatever to its benefits.

Circulation of water depends upon the difference in weight of two columns, the lighter being displaced by the heavier, so that to produce the same, we must have one column exposed to heat to make it lighter, whereas the other column must be separated or protected from heat, so that in nowise will its descent be prevented. A glance at the drawing shows that in the Perkins boiler no such effect could be produced. First, if we consider the central column as ascending, being exposed to the most intense heat of the fire, we also recognize the fact that the annular space cannot permit the free descent of the water as that also is exposed to heat, and in both the lateral and annular spaces steam is constantly formed, and the introduction of the diaphragm retards rather than accelerates the production of steam. Professor Bache says, "that the introduction of the cylinder or diaphragm caused violent foaming, so much so, that he was compelled to affix a funnel-shaped cylinder to the top of the vessel to prevent the water from being forced over its sides."

Now this violent foaming is the direct result of the division of the water, by which steam formed at the bottom, rising rapidly and expanding, carries solid water with it. That under such circumstances a certain quantity of fuel cannot evaporate as much water, is evident from the fact that heat being power, all power lost in forcing the steam through the water is so much heat wasted.

This other boiler, in which the desired conditions are proved, shows plainly where the Perkins boiler *failed*, and how a perfect circulation of the water may be obtained, causing a rapid upward and a slow downward current, and provision also made for the collection of impurities below the line of fire. That the two systems differ in the most essential points, is apparent, as well as that one fully accomplishes what the other merely attempted.

In conclusion I would lay down a positive law for the construction of steam boilers, which under all circumstances *must* be absolutely free from disastrous explosions.

1st. A steam boiler should be constructed of the largest practical number of units, so that the giving way of one of them under excessive pressure, relieves the whole.

2d. Each unit must constitute in itself a complete steam boiler, having its water space exposed to the fire, its steam space above the fire, and a receptacle for the collection of sediment below the influence of the fire.

3d. All heating surface must be vertical, or nearly so, at or nearly at right angles to the line of fire.

4th. Free and perfect circulation of the water must be secured by having thin films of water exposed to the heat, ascending, and large waterspaces not exposed to the heat descending, thereby also securing perfect separation of steam from the water.

Steam boilers constructed on these principles will, in the hands of even the most inexperienced persons, be free from danger, and at the same time insure the highest economy in fuel.

The daily recurring loss of life, the destruction of property, the insecurity of travel, together with the daily increasing number of steam boilers, must be my apology for occupying so much valuable time. If, by my labor of years in investigating this subject, I may be instrumental in saving but one human life, my labor will not have been in vain.

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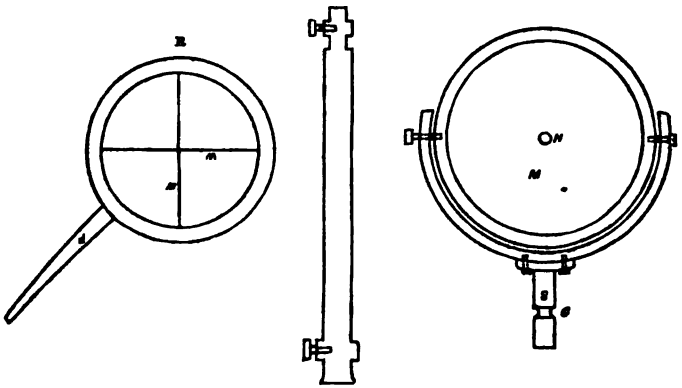
9. ON THE HELIOSTAT, AND METHOD OF RUNNING TRUE OR MERIDIAN LINES. By MICHAEL McDERMOTT, Civil Engineer and City Surveyor of Chicago.

THIS instrument consists of a mirror, pole, Jacob's-staff or steel rod, and a brass ring with cross wires. The annexed figures represent one used by me for some time, which has been made for me by Mr. Kratzenstein of Chicago, and cost but nine dollars.



The mirror (M) is of the best French mirror material, $3\frac{3}{4}$ inches in diameter, set in a brass bronzed frame or ring, $4\frac{1}{2}''$ outer, and $3\frac{3}{4}''$ inner diameter, and $\frac{3}{10}$ of an inch thick. This mirror is set into a semi-circular ring $\frac{4}{10}''$ thick, leaving a space between it and the ring of $\frac{2}{10}$ of an inch. Both are connected by two screws, one of which is a clamping screw. The semicircular, or outer part, is attached to a circular piece of the same dimensions as the outer piece, 1½″ long, and being part of an axis 9½″ long, into which there is a groove to receive the clamping screw from the tube or socket. This piece is marked S on figure.

The socket or tube is 8″ long, ½″ inner diameter, having two clamping screws, one to clamp the whole apparatus to the Jacob's-staff, or rod, and the other to allow the mirror to be turned in any direction. By these three clamping screws the instrument can be raised and turned in any direction. The back of the mirror is lined with brass, in the centre of which there is a small hole, opposite to which the foil is removed from the glass. A pole is set near the station to be observed so that the centre of a brass ring (R) will be over it. This ring is to be a little less than the diameter of the mirror, and so made as to have a piece of steel wire, 2″ long, attached to it, by which it can be fastened to the side of a pole which has several holes at four or six inches apart. The mirror is now set on the Jacob's-staff about forty feet behind the pole at station A, and making the heliostat and pole approximately in direction of station B, where the observer is ready to take an angle to A, and also provided with another heliostat. The ring is adjusted to the pole, and the centre of the mirror and that of the ring set as near as may be in direction of B. The assistant at station A makes the concentrated rays of the sun pass through the ring about ten minutes of time. If he does not receive a return flash from B, he changes the ring higher or lower and gives other flashes, and so continue till B returns a flash, then from B's flash A is able to adjust his heliostat more accurately. By a preconcerted code of signals B orders A to proceed to the next given trigonometrical station, and so continue.

The heliostat and pouch now exhibited weigh but 3¾ pounds.

A mirror of four inches will be seen forty miles, and one of eight to ten inches one hundred miles, where the stations will be sufficiently elevated.

MERIDIAN LINES.

All surveyors know that to reëstablish a lost line by means of the magnetic needle is impossible, as the annual and daily variations, combined with local causes, are obstacles too difficult to overcome. In Canada, magnetic surveys are not legal. There the bearings of all lines are determined astronomically, and consequently can be easily established at any future time with the *utmost accuracy.*

In this country the lines are supposed (since 1805), in the Western States to run North and South, and the lines running East and West to close within a limited number of links on the North and South lines. However, owing to *various causes*, the lines are found twisted out of position. Here, in order to locate a line, we must have its extreme points or corners, whereas in Canada one point is sufficient.

The present method of making surveys is very wrong and expensive, and is the cause of thousands of law suits respecting boundary lines. Let the surveyors only know that the Supreme Courts of many of our States have decided that where the lines in a deed are given South, North, East or West, such lines must be run due North or South. This is plainly laid down in Section 2 of the act of 11th Feb., 1805. (See United States Statutes at large, Vol. II, p. 313.) Hence appears the necessity of taking the true bearing of every line astronomically, or by referring it to some near line whose true bearing has been found.

In the Northern Hemisphere the stars in Ursa-minoris give us an opportunity of taking the meridian several times during the night.

It has been generally supposed by surveyors, that when Polaris and Alioth in Ursa-majoris are on the same vertical plane or line, that then Polaris is on the meridian; but such is not the case.

In Chicago, latitude 41° 50′ 30″, on the first of January, 1867, the bearing of Polaris, when vertical with Alioth, was North 0° 10′ 21″ East. (See McDermott's Civil Engineer and Surveyors' Manual, sec. 385, for method of calculation.) For a table showing the azimuth of Polaris when in the above position, in latitudes 2° to 70°, and for the years 1870, 1880, 1890, 1900, 1910, 1920, 1930 and 1940, see his table XXIV.

In the Southern Hemisphere there is no bright star near the South Pole. I have calculated the greatest azimuths of Sigma in Octantis, a star of the sixth magnitude, from latitude 1° to 70°, and for Polar distances 40′–45′ and 50′. Although the great Creator has not given any bright star near the South Pole, He has given Alpha in Crucis, and Beta in Hydri; the former in the foot of the cross, the latter in the tail of the serpent, and so situated that, when they are on the same vertical plane, the bearing of Alpha Crucis is very small, and its annual variation a mere nothing. For example I give part of my table XXV.

LAT.	YEARS.							
	1850	1900	1950	2000	2050	2100	2150	
12°	1′.12″	1′.15″	1′.19″	1′.43″	2′.15″	2′.58″	3′.53″	Azimuths, or bearings all South east of the meridian.
14	1 .12	1 .15	1 .20	1 .43	2 .16	3 .00	3 .56	
&c.	&c.	&c.	&c.	&c.	&c.	&c.	&c.	
20	1′.14″	1′.17″	1′.25″	1′.48″	2′.20″	3′.6″	4′.2″	
22	1 .15	1 .18	1 .26	1 .50	2 .22	3 .8	4 .4	

From this it appears that in latitude 12° it will require fifty years to make a change of three seconds of a degree in azimuth; also a change of 10° in latitude to make the same small difference. The placing of these two bright stars could not be the work of chance or accident, but that the cross, the emblem of and guide to salvation, should be an infallible guide in Surveying and Navigation, and there placed by the Great Omnipotent Architect, as the most perfect guide now known.

Table XXIV in McDermott's Manual shows the azimuths of Polaris when vertical with Alioth in Ursa-majoris for 1870, and for every ten years after till 1940, and for every 2° of latitude from the equator to latitude 40, and then for every degree from 40° to 70°.

EXAMPLE IN BEARINGS ALL NORTH-EAST.

LAT.	DATE.		
	1870	1880	
40	10′.25″	11′.22″	Nearly a change of one minute of a degree in ten years, and about ten seconds in a change of 1° of latitude.
41	10 .34	11 .32	
42	10 .44	11 .42	
43	10 .58	11 .53	

Time of certain North circumpolar stars being at their greatest azimuths, or elongations from the meridian, on the first of January, 1867, and of Polaris and Alioth in Ursa-majoris being vertical with one another.

		TIME.
		h. m.
Alpha Ursa-minoris (Polaris),	East of the Meridian,	0.31
Beta " (Kochab),	West " "	1.10
Beta Draconis,	" " "	1.49
Gama "	" " "	2. 8
Alpha Cassiopeæ,	East " "	2.18
Zeta Ursa-minoris	West " "	2.22
Epsilon "	" " "	3.48
Delta "	" " "	5.19
Polaris, vertical	with Alioth,	6. 5
Alpha Cephi,	West of the "	6.38
Gama "	" " "	10. 2
Phi Ursa-majoris,	East " "	10.34
Alpha "	" " "	12. 2
Polaris,	West " "	12.21
Beta Ursa-minoris,	East " "	15. 4
Epsilon "	" " "	15.48
Xi "	" " "	16.43
Phi Ursa-majoris,	West " "	17.15
Delta "	East " "	17.43
Beta Draconis,	" " "	19.37
Gama "	" " "	20.10
Gama Ursa-majoris,	West " "	20.24
Alpha "	" " "	20.21
Alpha Cephi,	East " "	22.25
Gama "	" " "	23.38

The above is to the nearest minute; but in the table it is given to seconds; also the meridian passage and time to greatest azimuth.

In this method an error in the latitude of half a degree would be about one-half a minute in azimuth of Polaris in latitude 41°. But as the latitude can be taken with little delay, there need be no cause of any sensible error, and lines can be run much closer than now. I calculate the time from the meridian passage to the greatest elongation of the star, and its altitude when at that position. This requires to have the true time, which I find by a new method, as follows:

I calculate the time to or from the meridian passage to the greatest elongation reduced to the sidereal time of station, which reduce to mean time. I next find the true altitude of the star when at its greatest azimuth, to which add the correction for refraction from my table XVII. I take the temperature of the thermometer and approximate that of the barometer, which ranges in clear weather from 29½ to 30. I add this correction for refraction, to find the apparent altitude. I observe when the star is at this altitude and compare it with the calculated time, and thus correct mine, by which I am able to know the precise time of the required star being at its greatest azimuth, and having the altitude as a check, there can be no method to give greater accuracy.

To find the greatest azimuth = Rad. 10 + Sin Polar Distance, less cos. Lat. = sine of greatest azimuth.

Altitude at greatest azimuth = Sin. Lat. + 10, less cos. Polar Distance = sine of the altitude.

To find the time from meridian passage to the greatest azimuth. Tan. polar dist. + tan. Lat., less 10, = cos. hour angle in space which change into time, gives the sidereal time. (Polaris and Alioth are always on opposite sides of the pole.)

In taking the bearing of a line we have on the object end of our telescope a reflector made thus:—a piece of sheet brass ½′ wide is made to fit the telescope, as A. C is made with an elliptical hole and fastened to A, making an angle of about 45°. There is a space between A and C about ⅛″. This is put on the telescope. The assistant holds a lamp by which the light can be thrown on the surface C around the hole, and regulated without the least difficulty. We observe the star in the required position, lower the telescope and make a permanent mark in that direction, about ten chains in advance. Then we drive a picket with its top finely pointed, or a stake and nail, to be used on the next day on which we set off the angle required by the instrument, by repeating it two or three times; or we measure the distance from the instrument to the picket and there set off the tangent of the required angle, thus insuring the greatest accuracy. Or let us suppose the angle to be set off = 11° 43′. We set off the 11° and measure ten chains, calculate the tangents of 43′, and set it off with a foot rule. Hence, if we set this small tangent off at twenty chains from the stake, we can run *a distance of six miles to an inch*, by using fine pointed pickets whose tops are set in range. I find whitewashed laths excellent. At the station or picket in advance we use a steel rod five feet long, on the top of which is a disk of sheet iron or pasteboard, behind which our assistant holds his lamp till we see the light at the point or apex of the disk. The open space in disk is one inch at bottom, three inches high, and coming to a point. At the top a tube is attached to fit the rod.

10. On the Principles of Statistics as applied to the Census. By Franklin B. Hough, of Lowville, N. Y., *Superintendent of the New York State Census of* 1855 and 1865.

The near approach of the period fixed by our Constitution for another National census will justify, at this time, a careful consideration of the methods hitherto employed in making enumerations of this kind, and some of the details of organization that should be considered, in order that the results may be such as may command the public confidence.

I do not propose to undertake an historical review of census systems, nor do I deem it necessary to present arguments to prove that unless the inquiries of the census are carefully and judiciously arranged, and thoroughly and uniformly reported, the deductions therefrom will be comparatively worthless, and even less trusty than mere estimates founded upon general observation.

It may be proper, in passing, to notice the fact, that although returns of population more or less accurate had been previously made through government agencies, the United States was the first country in modern times that provided for a precise and full return of the number of its inhabitants at decennial periods. The necessity of an exact basis upon which representation could be equalized, led to the provision requiring that "within three years after the first meeting of the Congress of the United States, and within every subsequent term of ten years," an actual enumeration of the population should be made in such manner as the law might direct. Under this authority we have had eight returns, commencing with 1790.

In Great Britain, the first regular census was taken in 1801, and in each succeeding year whose unit figure is 1, a full census has been reported. In France, a census was taken in 1801, 1806, 1821, and has been continued at intervals of five years since. The custom has since been adopted in most European countries, in many European colonies, and in several of the States of our Union.*

*Of the thirty-seven states, all but five have a clause in their Constitutions requiring or permitting a census. Of these, twenty-eight take a census at intervals of ten years; one in eight; one in seven; one in six; and one in from four to eight

The oldest provision for a stated census, by State authority, is found in Pennsylvania in 1776 and New York in 1777, where, under the first Constitutions a septennial return of the classes represented was provided for. Since 1825 the census of New York has been at ten-year intervals, and for the last two periods (1855 and 1865) the schedules of inquiry have been more extended, and the deductions more thorough and systematic, than any hitherto published by State authority. As these two returns were made under my direction in every stage of progress, from the preparation of blanks to the distribution of the printed volumes, I shall have repeated occasion to refer to the operations and methods there employed, and to state some of the results of experience derived from these labors.

It will be convenient to notice our subject under three principal heads, viz.:

1st. As to the time fixed or allowed for making the enumeration.

2d. As to the blanks used, and subjects of inquiry.

3d. As to the agencies employed, and mode of compensation.

(1.) The earlier American census returns had no fixed point of time to which they referred, being taken on a given year. The first four census returns under the present National Constitution, were made with reference to August 1st. The last four with reference to June 1st of the census year. They are now returnable on or before the first of November. In New York it is also taken with reference to the first of June, and one month is allowed. In Wisconsin, from the same day, and two months are allowed; and in most of the other States, where a census is taken, the time fixed is the first of June or first of July.

The British census of 1861 was taken on the 8th of April, and referred to the population on the night of April 7th. Two or three previous returns in that kingdom were made also on a single day, but at different periods of the year. A want of

years. But a census is not actually taken in every State whose Constitution says it may be done. In eighteen States, the results of the National census become the basis of State equalization for their Legislatures. In Arkansas this will be done in 1880 and after.

uniformity in this regard is much to be regretted; and the summer season has this grave objection, that many persons, especially those in easy circumstances, are then temporarily away from their proper homes. It is probable that no date could be selected preferable to January 1, or at least early in that month, could other details be arranged properly. At this period of the year most families are together, and the results of the year previous are most easily obtained. Business accounts are then usually posted, and the earnings of fields and factories are fully known. More than all this, it is a season of leisure, and with proper announcements through the press, people would be able to prepare answers in readiness when called for. The adverse reasons against this time of year would be inclement weather and short days. The first of March would be the date next preferable, as the days are then of average length people are at home, the removals of residence have generally not been made, and it is a time of leisure with farmers.

In the British census as now taken, in several of the English colonies, and in some countries of Continental Europe, the census is taken in a single day, by immensely increasing the number of agents employed as Enumerators, and carefully preparing for the labor of that day. The only instance of an attempt to thus shorten the time for taking a census in this country, was in a census of the District of Columbia on the 11th of November last, taken under the auspices of the Department of Education, by authority of Congress, and of the local authorities. In this instance, through the agency of the Metropolitan Police, a most thorough and satisfactory result was obtained. A large majority of the returns were made on the first day, and the whole within four or five days from the one appointed, and so far as I have been able to ascertain, no census has ever yet been attempted where the last returns did not linger behind the period fixed by law for final completion.

In the highly and justly praised census of Great Britain, and with the facilities afforded by a well organized Registration system, the first preliminary statement of results were not laid before Parliament until two months after the census day.

It is, for obvious reasons, desirable to lessen the period of enumeration as much as possible, by a careful arrangement of nearly equal districts; but it should never be forgotten that this wide distribution of appointments also increases the chances of incompetent Enumerators, more especially where the whole system must be extemporized for the occasion, as in the United States, and where so wide a diversity of subdivisions prevails in the different States. The township system in one, counties only in another, and the great range of subdivisions found in cities and wards, boroughs, precincts, parishes, districts, etc., presents a problem full of difficulties when attempting to adopt one plan of subdivision adapted to all. It is worthy of remark, that our system of public surveys and sectional subdivisions in the newer portions of our Union, afford precise and convenient designations for census districts.

(2.) As to the blanks used and the subjects of inquiry. We may classify the schedules hitherto employed in the census as follows:

1st. General blanks for family names.
2d. General blanks for individual names.
3d. Householder's schedules.
4th. Special blanks.

The earliest blanks used in taking the census allowed one horizontal line for a family, and the several columns were arranged for entering the number of persons in each, and such limited information relative to color, sex, age, occupation, etc., as this crude method would allow. It is obvious that no deductions concerning age, place of birth, occupation etc., could be attempted from these returns, except so far as provided for by separate columns, and that in order, in the remotest degree, to approach the standard of modern science in these details, the blank would need an immense horizontal extent. In fact, the United States census of 1840, the last one attempted by our government on this plan, provided headings extending through several yards of columns for entering these varied items, and finally obtained but a crude result at last.* In several of the States, as in Michigan, Wisconsin, etc., this primitive method is still practiced.

*The blanks of 1840, actually contained seventy-two columns for personal inquiry, thirty for mines, twenty-seven for agriculture, four for horticulture, eleven for commerce (including "butchers"), seven for fisheries, six for products of the forest, and one hundred and twenty-eight for manufactures. Total 285. Allowing half an inch of width as an average, it would reach nearly across twelve feet of space.

(2.) *General blanks for individual names.* This greatly improved plan of arrangement employed in 1850 and 1860 by the Federal Government, and now commonly adopted, provides a horizontal line across the sheet for each name in every family. In the various columns are entered the specific sex, color, age, civil condition, place of birth, occupation, etc., of the person as given by a member of the family to the Enumerator. The adoption of this plan constituted a prominent era in the history of statistics; and the student of social science, or political economy, who attempts to obtain comparable and satisfactory data of earlier periods, will find nothing but uncertain and general statements. The answers being reported in precise terms, allows this form of blank every degree of perfection that can be conceived of or desired in a census.

(3.) *Householder's schedules.* This form of blank, like the last, is arranged for entering the details relative to each individual, but is limited in size to the wants of single families, is left to be filled out by the head of each family, to be delivered on the day appointed for the census to the persons serving as Enumerators, and should contain printed instructions, with examples of filling up adapted to common understanding. It is the form used in the census of Great Britain, and most British colonies, and the kind used in the census of the District of Columbia already noticed.

(4.) *Special blanks,* adapted to particular inquiries, and varied to meet the peculiar nature of the subject to which they apply. These blanks were first employed on an extensive plan in the census of New York, in 1865, and with great success. They were of twelve varieties, and applied to the most common and generally distributed manufactories and institutions, concerning which details were required.

With reference to the inquiries that should be included in a census, an unfortunate tendency has prevailed of loading the columns with questions upon a wide range of subjects, by which the attention of the Enumerators has been distracted, and their labors prolonged; and there can be no question but that the accuracy of results has been seriously impaired from this cause.

The British census is a return of the population and nothing else. The people are reported in the varied conditions of sex, color, age, nativity, civil condition, occupation, religion, etc., but the products of agriculture and manufactures, the statistics of churches, schools, newspapers, public institutions, etc., are left to be ascertained by other methods of inquiry, and are very fully set forth in their proper place. But with us all these and more are crowded into the decennial census; but in a vast number of instances but partially returned or wholly overlooked. While there may yet, for a season, be good reasons for retaining these questions, the tendency should be to gradually find other agencies to which to refer all extraneous labors. Under this view, in arranging the blanks of the New York census of 1855 and 1865, I left statistics of schools and academies to the Department of Public Instruction, and the Regents of the University; the carrying trade to the existing agencies of the Canal and Railroad systems; of insurance and banking to their proper Departments; and of incorporated institutions to the stated means of report provided in their charters.

With reference to the personal census, the blanks to be complete should embrace every name, with a precise return of color, sex, age in nearest years, or if under one year in months; civil condition, county and State, or particular country where born, occupation or means of support, whether a voter or an alien, whether deaf and dumb, blind, insane or idiots, with indications of the causes if known, and degree of education, at least so far as whether able to read or write. It should also return orphans under fifteen years of age, and *should not* attempt to get, by personal inquiry, any statistics as to pauperism or crime. These relate to disreputable classes, usually so considered, and attempts at concealment would impair results. Get these facts if desired, from official records, and these only.

I have observed that the people will generally answer the inquiries of the census cheerfully, fully and correctly, if they understand them. Exceptions to this rule are only found where the consequences of the return may affect the personal liability of the informant. The last census of New York for 1865, and of other States for the same year, were impaired in value by evasive answers relative to ages of males, through the fear of a military draft; and had the war continued a few weeks longer, it would have been altogether impossible to make the enumeration, notwithstanding the imperative terms of the Constitution. The inquiries concerning ages and nativities, which the census includes, are too nearly like military enrolments to be distinguished by the common classes in times of war, and resistance against the unsupported labors of the Enumerators would surely attend their efforts. But in times of peace I apprehend that evasion or refusal may be considered so rare as to scarcely impair the general accuracy of results. Happily we have no cause of trouble in this regard in prospect, and with growing intelligence among the people the jealousies alluded to will pass away.

With reference to agricultural statistics, the ideal of perfection is realized in the French cadastral surveys, in which the soil, its contour, and the uses to which it is annually applied may be known. With us we can only hope to realize trustworthy results through a well organized system of State and county agricultural societies, reporting to a central Department, and provided with authority and means for obtaining annual statements of cultivation and products. One competent man in every school district could be found willing, for a small sum, to devote a day or two to this labor. The experiment has been tried by the New York State Agricultural Society, but, except in cases where particular efforts were made, it proved a dead failure, and for the best of reasons: it paid nothing for the labor, and had no penalties for neglect. Even the long delayed volume of Transactions promised to all who reported, was received by but few; and the attempt has been nearly or quite abandoned.

A passing word with reference to blanks for agricultural statistics:—They have usually been on large sheets with separate columns for each of the principal crops; their area and quantities, number and value of domestic animals kept for farm purposes, and sometimes blanks for unenumerated crops or products. To be in the least degree complete, these columns were necessarily multiplied greatly, and in the New York census of 1865 extended to 121 in number. The answers to these inquiries were necessarily given in a hurried manner and by general estimates in most cases. Consider, for a moment, the superior advantage of using a sheet not larger than a letter page, to be left with the farmer to be filled by him at his leisure, and called for a day or two afterwards. On the face of this sheet there should be a printed form with name of person reporting, the town, county or district, number of acres improved and unimproved, estimated cash value of land and of buildings, and then a blank sufficiently wide to enter for each crop the acres sown and bushels harvested, and cash value, with blanks for entering the number and value of farm stock, and any farther information that might be required. On the back of the blank could be printed full instructions, and an example of filling up.

I believe that in most instances such a blank would be cheerfully and carefully filled up by the farmers themselves, and that the saving of time would overpay the extra cost. In condensing these blanks for publication, the quantities and values would need to be posted off into columns; but this, under the discipline of a well ordered office, would be rapidly and correctly done, and the columns thus prepared would be added with much less trouble than the straggling, crooked, and often defective columns of the present census blank.

As to manufacturing statistics, the official returns of our revenue system are supposed to give accurate data of some. The associated interest of manufacturers of iron, leather, lumber, etc., have, in some instances, led to inquiries and reports embracing more systematic and accurate statements than can ever be obtained through the agencies of a census; and we believe that there is more hope of improvement in the voluntary efforts of the parties interested in knowing the extent and distribution of their own particular branches of industry, than in anything that can be gained through a census. Instead of the general blanks hitherto chiefly used, special blanks, on the plan of those used in New York in 1865, might be employed to great advantage, and for other branches not thus provided for, a sheet of printed forms and instructions analogous to that suggested for agriculture might be employed.

But until some better disposition can be made of the statistics of agriculture and manufactures, they must be carried by the census as best may be. With suitable penalties for neglect or refusal to answer, and sufficient intelligence to understand that there is no taxation behind them, the question is simply one of time and expense.

As to personal returns, the changes wrought by the recent war require that certain inquiries should be made with reference to the conditions of those made free by the success of the Union arms. By a single narrow column we could designate, opposite the name of every colored person, whether free before the war or freed by the war. There should also be an inquiry, applicable to all classes and everywhere, as to civil condition, and whether voter or alien. The census of the District of Columbia of November last had columns to designate the ancestry or parentage of each person; years resident in the District; whether permanent residence was intended; whether an owner or renter of real estate in the District; whether in the employment of the United States; and if so, whether civil, military or naval; name of school and months attending (if a scholar) during the past year; and the number of orphans, in addition to the usual questions of the census.

A characteristic feature of the English census is an inquiry relative to the religious creed or faith of individuals, and this has been copied in Canada, Nova Scotia and other colonies. But in this country I would consider such a question as quite out of place in a census, and that the least approach to the delicate subjects of religious faith or political creed should be entirely excluded from the schedules; and still more, that the agents employed should be instructed to avoid even the appearance of seeking information upon these subjects, in which it is the high privilege of the American citizen to hold his own opinions, and his reasons therefor, exempt from inquiry, and free from accountability.*

The concurrent experience of all who have had the charge of census labors would doubtless confirm this statement, that no attempt should be made by the Enumerators to sum up or classify their own labors, unless in a general total of per-

* Since this paper was read, a census of the city of Baltimore was attempted on the plan of Householders' Schedules. But an unfortunate question as to "Religion," ruined the whole. The returns are said to have brought in more than sixty thousand short of the real population. The work was thrown away, and a new enumeration on the rudest plan of primitive inquiry as to numbers, without any blanks of any kind besides common pass books for totals, was substituted.

20

sons, for early announcement of proximate results. In the earlier returns of New York the footings by towns were reported to the county clerk, who prepared a table for his county and forwarded this to the Secretary of State for publication. The work thus came together finished, but so full of discrepancies as to justify the most positive belief in errors, while there existed no means of correcting them.

We now come to notice the agencies employed and mode of compensation. In England, as already noticed, the system of registration of births, deaths and marriages, constantly organized and in operation, and kindred in purposes, affords a valuable medium for census purposes. Of registrar's districts in 1861, in England and Wales, there were 634. These were divided into 2194 subdistricts, and these again into 30329 enumeration districts. In the first two of these there were permanent officers who knew their districts, and the proper persons to employ, and who could lay out the enumeration districts carefully, and very nearly equal in amount of labors to be done. In towns, 200 houses, and in rural districts fifteen miles of travel were assigned to each person; and special provision was made for reporting those living on vessels; the absent marine; the royal navy, and persons employed in navigating the inland waters of the kingdom.

Our experience of the householder's schedule and one-day system in the District of Columbia was not strictly based upon an equal division of territory, as in most cases the members of police took the census of their customary patrol beats. These were smaller around the centres of trade and travel, and near markets, theatres etc., than in the quiet and respectable quarters of the better classes of inhabitants; and their attention was, in some instances, partially devoted to other duties. They were, however, able very readily to collect blanks from 200 houses in a day. But in the thinly settled country outside of the cities, although our men were mounted, their labors lingered longer, and were more difficult than in densely settled localities. I would think ten miles a day sufficient for one man to do thoroughly, where he had only to collect the blanks previously distributed. In New York, we had 1744 Enumerators in 1855, and 2332 in 1865, besides those appointed to Indian reservations. Their labors varied from 100 to 10000; and when all the data were written by the agent at the time of his visit, if he reported 300 names he did a full day's work. This is on the supposition of a concurrence of favorable circumstances; a dense population of the common or lower classes, with no time lost in travel, and none in delay. It would require but little experience to ascertain that at least five times as many persons could be reported in one day, in a dense crowded quarter than in a fashionable street, where delays at the door—the master at his business—the lady at her fashionable calls—"not at home"—"call again," and a dozen other excuses, delay the work of enumeration. But where Agricultural or Industrial statistics also require attention, the work is farther delayed. On a general average probably fifty or sixty names would be found a day's work.

In some of the States the census is reported by the assessors. I consider this highly objectionable, because it associates with the enumeration a suspicion of taxation, which will tend to impair the result. It is a well established principle in this class of statistics, that the inquiries, by whomever and however made, should be as much as possible independent, and understood to be only as a measure undertaken for procuring correct information for the general purposes of government. Hence the proposition for the employment of Internal Revenue Officers for taking the census is of all others, the most objectionable. These officials have hitherto never made a call to ask a question, unless it was soon *followed by a tax.* Evasion and concealment would be the inevitable result.

In the general government the agents have been hitherto employed under the marshals of the District Courts, and have been usually called "Census Marshals." But the memory of "Provost Marshals" has made the term so suspicious in the minds of the masses of our population that a change is imperative. The term "Enumerators" has been in many cases used, and as it correctly describes the duties of the office it should hereafter be employed.

Two modes of payment have hitherto been adopted. In most of our State and local census systems a *per diem* allowance, fixed by law and sustained by affidavits of service, has been given from the State or county treasuries, upon the audit of supervisors or other financial officers. In New York, as now fixed by law, Enumerators are allowed $3.00 a day for the time actually and necessarily employed in taking the census and making one copy. The duplicate copy, reported to the Secretary of State, was, in most cases, neatly and handsomely prepared, and often by a female hand. Payment is made to depend upon acceptance at the central office; and this fact proved of the highest value by impressing an abiding sense of responsibility upon all, while it afforded a convenient means of enforcing attention to the revision of errors and the filling of omissions.

In the general census, a *per capita* rate is allowed for population returns, a specific rate for each manufactory, and for areas of agricultural districts reported.

It is but too well known that shameful frauds have been hitherto practiced under this method, where fictitious names could be multiplied in the large cities and in densely settled localities, with very little risk of detection, and it is suspected that an effort to secure unjust claims to representation in Congress, may have been connected with these frauds. Viewed in every light, the *per capita* plan of payment, appears to me more objectionable than a fixed allowance for the time employed.

But of all contrivances for covering fraud, under the cloak of science, the law of 1850 (still in force until changed), relating to the pay *for mileage* is the most ingenious. It gives ten cents a mile, to be ascertained "by multiplying the square root of the number of dwellings by the square root of the number of square miles in a district." Let us examine the operation of this highly "learned" method:

Of the series, ...	1,	4,	9,	16,	25,	100,	10 000,	1 000 000,	etc.
The square roots are, ...	1,	2,	3,	4,	5,	10,	100,	1 000,	etc.

in which it is apparent, that the relative proportion of the root to the number itself, rapidly diminishes at first and goes on forever approaching zero without ever reaching it. In short, the curve is a *Parabola*.

If an Assistant were assigned a county as his district, he would be justified in seeking for a description of its boundaries in the statutes. These extend the boundaries of all the counties of New York, adjacent to Lakes Erie and Ontario, to the State line in the middle of the lake. I believe this is true of all the States bordering the great lakes, in which there would be found the legal sanction for claiming the benefit of mileage for the areas under water. But on shore, and especially in thinly settled States, the injustice of this rule becomes striking. When reduced to *per capita* rates, it allows twenty-six mills in Hamilton Co., N. Y., six mills in Herkimer Co., and four mills in Oneida Co. The factor of square root of square miles for Texas, is nearly twice that of all New England together, while its total population is not a fifth part as great.

Again, the rule of compensation to Marshals, for supervising the census, is absurd. If the district be under a million, it is at the rate of $1.25 per 1 000. If it exceeds a million, the rate from the beginning is $1.00 per 1 000. If he reports 800 000, he gets the same as for 1 000 000. For one *less* than a million he gets $125.00 more than for one *more* than a million.

The various intermediate agencies between the Enumerators and the central office should be suitably paid for services rendered, and at least one competent, active and honorable man should be assigned to each Congressional district, to select competent Enumerators, and exercise a vigilant supervision over them.

In every case, a careful revision and inspection should precede payment, and the central office should have power to withhold compensation until all needful corrections or deficiences were supplied.

With a suitable organization, a careful selection of a local superintendent in each Congressional district, and a like selection by the latter of persons, chosen on account of their intelligence and integrity, in every township, or, still better, in every election precinct or school district; with sufficient time to instruct every man uniformly in his duties, and a set of blanks, precise and simple in form, for their use, we might reasonably hope that our national census, upon which such mighty questions of public policy and private enterprise depend, could attain a fair degree of accuracy and be entitled to our fullest confidence.

I have observed that the character of returns from a given county or ward, have a general similarity as to fullness and care, or the opposite; and that these characteristics depend much upon the character of the men by whom nominations of Enumerators were made. In large districts men were recommended for the office highly qualified, diligent and punctual in their labors, while in other cases, the class selected proved to be illiterate and incompetent as a class. In the former case the lists were prepared under an impressive sense of duty, and with an honest desire to serve the public interests, while in the latter the opportunity was seized upon to reward partizan services, or to promote some personal object or political intrigue. This experience has its moral, and should teach a lesson of wisdom, if we would arrive at a trustworthy result.

Statistical labors judiciously conducted, involve two separate processes, which cannot be carried on together without risk of error and loss of time. The recording of facts, with careful attention to their accuracy, constitutes the first division of these labors, and to these we have limited ourselves in this paper. The condensation and preparation of these facts for use, involving the three principal operations of tallying, posting and adding, belong to the office labors, and are so entirely within control that errors are needless and unpardonable. In many cases the work if correct will balance itself in the general result, and where this mode of proof is not practicable every process should be revised before being checked as right. A rule allowing a fixed sum to be deducted from the wages of a clerk committing an error to be paid to the one who detects it on revision, would check careless habits and secure vigilant attention. The labors of classification may in many cases be simplified, and mechanical devices are applicable in tallying and adding numbers, that would greatly facilitate clerical labor and relieve the clerk from that constant mental labor so exhausting when continued daily for long periods.

A daily report of work done by each clerk and a system of precise responsibility by which an error, if found, can be traced to its author beyond doubt, would constitute an essential feature in a well regulated office.

The classification should always have reference to comparability, with similar data of other period or locality, and if new subdivisions are adopted they should be so arranged that by their combination they would readily admit of such comparison.

11. REMARKS ON THE SECULAR VARIATIONS OF THE PLANETARY ORBITS. By J. N. Stockwell, of Brecksville, Ohio.

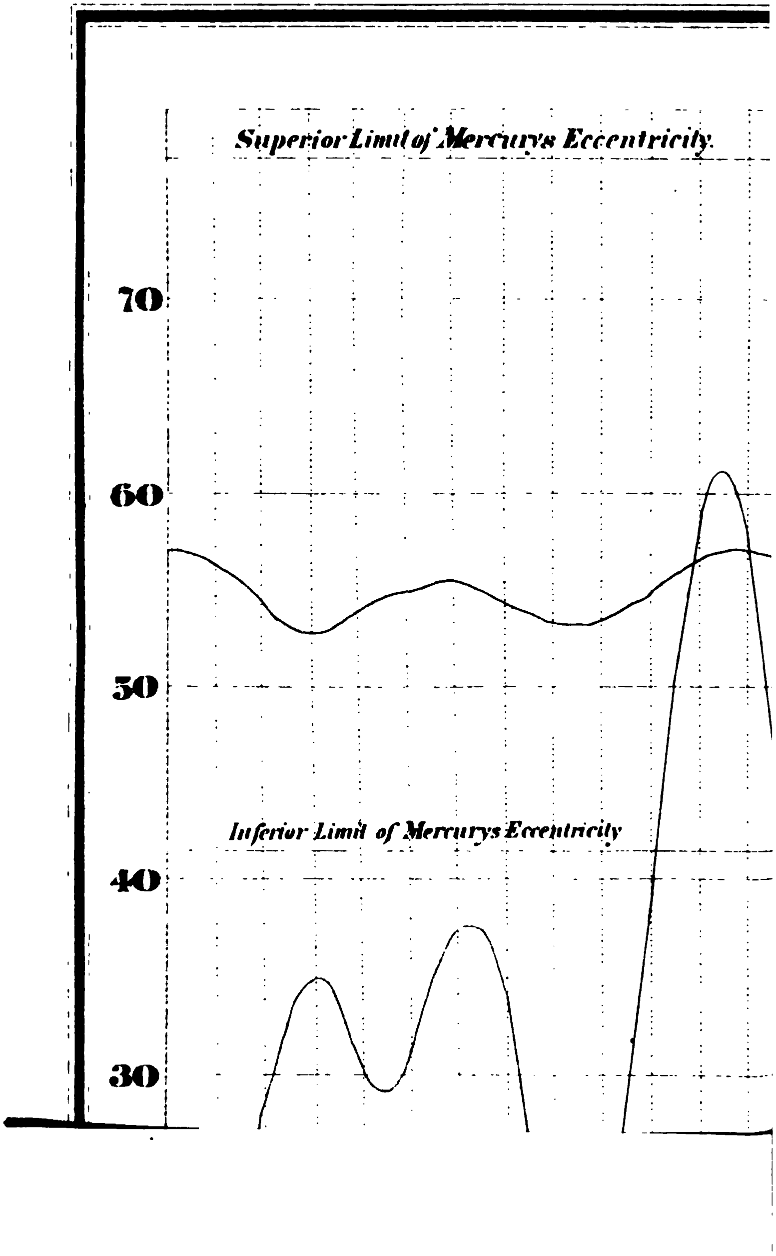
THE secular variations of the elements of the planetary orbits, is one of the most interesting and important problems which the theory of the universal gravitation of matter presented for the consideration of mathematicians. This problem is one of immense difficulty, for, in order to determine the variation of the elements of a single planet, it is necessary to know the simultaneous variations of the elements of all the others. It is this circumstance which invests the problem with its greatest difficulty, and renders its rigorous solution one of the most valuable contributions to celestial mechanics.

The researches of La Grange and La Place, towards the close of the last century, made known the laws which regulate and control the forms and positions of the planetary orbits; and these laws are embodied in several theorems of great elegance and generality. It was shown that all the variations of the elements of the planetary orbits, are confined within narrow limits; and that the stability of the planetary system is maintained by the forces of gravitation. The analytical investigation demonstrated the stability of the system; but did not, of course, make known the limits of variation within which each element would be confined; since the actual variations depend upon the relative values of the masses of the different planets of the system. It was therefore necessary, in order to determine the actual condition of the planetary system, during all past and future ages, to substitute the values of the masses and elements corresponding to each planet, in the analytical formulæ, and by means of a laborious process of elimination determine the particular values of the constants corresponding to each planet. The number of constants to be thus determined, amounts to eighty, for the determination of the eccentricities and perihelia of the eight principal planets; and eighty more are required for the determination of the nodes and inclinations. A knowledge of the actual condition of the planetary system at any period of time, therefore, requires us to determine the values of one hundred and sixty constant quantities,—thirty-two of which are indeterminate by analysis, but may be derived from the values of the elements at a given epoch.

La Grange made the first successful attack on the problem of finding the approximate numerical values of the constants which determine the variations of the elements of the seven principal planets which are within the orbit of Neptune. But his solution was based on values of the masses of the planets which differed much from the truth. His determination of these constants could therefore be considered only as provisional, and authoritative only until more correct values of the masses of the planets should be arrived at. About half a century from the time of La Grange's investigation, when more accurate values of the planetary masses had been obtained, Le Verrier undertook a new determination of these constants. He very skilfully determined all the constants, together with the coefficients of any supposed errors in the masses of the planets. But his determination of these coefficients was founded on two theorems which are true only on the supposition that the assumed masses are extremely near the truth; or, in other words, on the supposition that the possible errors of the masses are infinitely small. This investigation of Le Verrier's was made about thirty years ago; and the values of the masses which he used, are some of them, considerably different from the values received at the present time. These successive corrections to the masses, together with some small corrections to the elements of the orbits, taken in connection with the discovery of the planet *Neptune*, was a sufficient incentive for submitting the question to a new discussion. I therefore, some ten years since commenced the investigation, using as the groundwork of my solution the masses and elements of the orbits of which are used in the construction of the *American Ephemeris and Nautical Almanac*, with the exception of the mass of the *earth* and the elements of *Neptune;* and have since devoted such time as I could spare from other duties, to the prosecution of this work, which is now substantially completed.

I purpose now to give a brief abstract of the results of my investigation.

Since I completed the determination of the constants, I have


Superior Limit of Mercurys Eccentricity.
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Inferior Limit of Mercurys Eccentricity
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computed the eccentricities and places of the perihelia, of the planets *Mercury* and the *Earth*, at intervals of 10 000 years, during a period of 2 000 000 years; and have drawn a chart exhibiting the relative eccentricities at all times during that period. The eccentricity of *Mercury's* orbit will always be confined within the limits of 0.231 994 and 0.123 274; the minimum being a little more than one-half of the maximum. But it is not probable that these extreme limits of eccentricity will ever be reached, since the coefficients of the *time*, in the expression of that element are incommensurable, and consequently the *co-sines* of all the angles can never be equal to ± 1, at the same time. During the 2 000 000 years which I have charted, the eccentricity has been confined within much narrower limits,—the greatest maxima being only 0.2072, or slightly greater than the present value of that element, and the smallest minima being 0.1586; so that the whole variation during 2 000 000 years amounts to only 0.0486. We see by the chart, that the variations of its eccentricity consists of a series of small elevations and depressions, having a comparatively short period; but these small elevations are at present combined so as to produce one grand and uniform swell of the eccentricity, during a period of 400 000 years.

The mean annual motion of *Mercury's* perihelion is 5″.509-545. It therefore performs a complete revolution in the heavens, in about 235 200 years. And the greatest quantity by which the perihelion can differ from its mean place amounts to only 17°.

The superior limits of the eccentricity of the orbits of the planets *Venus*, the *Earth* and *Mars*, are respectively 0.073 930, 0.0 693 888 and 0.1 402 156; and their inferior limits are each equal to nothing. The orbits of these three planets are the only ones in the solar system which can become circular through the agency of the forces of gravitation. The perihelia of these three planets can have no *mean motion*, since the ellipticity of the orbits is a vanishing element, and the position of the transverse axis becomes an indeterminate quantity the moment the ellipticity disappears. It is therefore possible for a new ellipticity to commence, with its perihelion diametrically opposite the point at which it disappeared; and in such a case the transverse axis would apparently revolve with an infinite velocity.

The eccentricity of the earth's orbit is one of the most interesting and important elements of astronomy. Not only does it have an important influence in modifying the amount of light and heat received from the sun in the course of each year, but it also has an important bearing in matters pertaining to the chronology and antiquity of the human race. For the inequalities which that element produces in the sun's apparent motion are so great as to force themselves upon the notice of observers, as soon as they pass from a barbarous to a civilized and enlightened state. And when an inequality is once detected it is carefully observed, and its maximum value accurately ascertained and recorded. But since the eccentricity is itself a variable quantity, the inequalities which it produces in the sun's apparent motion will be different at different periods of time; and no two observers will obtain the same values of these inequalities unless they are exactly, or very nearly, contemporaneous. The values of these inequalities, as recorded in the annals of a nation, therefore, serve as an accurate chronological record of the epoch of the observations on which the recorded inequalities are founded. If we, therefore, know the values of these inequalities at two different epochs, we may use them to determine the interval of time between them, when we know the exact law according to which the eccentricity varies. On the other hand, when the inequalities and epochs are both known, we may use them to correct any assumed law of variation of the eccentricity.

As the question relating to the antiquity of man, has been so thoroughly ventilated by several members of this Association, from the study of the fossils of prehistoric ages, any farther allusion to the subject at this meeting might seem improper and out of place. But I trust you will pardon me for alluding, not simply to the fact of his existence during many centuries antecedent to the epoch of his creation, according to the usually received chronology, but to the fact that he had made great advances in civilization and science, at an epoch antedating European records, and the heroic ages of Greece and Rome, by more than two thousand years. These records of his civilization and scientific culture, I need scarcely say, are based on astronomical elements of a former age,—elements which are perpetually changing. And it will be one of the grandest triumphs of astronomy, to be able to reconstruct a chronology which the lapse of ages has in great measure obscured.

About a century ago the French astronomer, Legentil, was sent to India to observe the transit of *Venus* over the sun. He there became acquainted with a learned Brahmin, and from him obtained a set of tables and a knowledge of Hindoo astronomy. The astronomical tables he carried with him to France, and they were subsequently published in the Memoirs of the Academy of Sciences of Paris. These tables contained the various astronomical elements which are necessary in order to calculate the positions of the heavenly bodies, at an epoch 3 102 years before the Christian Era. The great antiquity of the reputed epoch of these tables induced astronomers to reconstruct tables for that epoch, by applying to the existing elements the changes which should have taken place since that epoch, according to the theory of universal gravitation. The identity of the resulting elements, when thus corrected, was not regarded by scientific men as sufficiently close to warrant the conclusion that they were founded on the state of the heavens at that epoch; and they were therefore regarded as mere fanciful creations of the imaginative Hindoos.

But a question of so much interest and importance ought not to be hastily decided. All the circumstances which can possibly have an intimate or remote bearing on the question should be carefully considered, and due weight allowed them in the final summing up of evidence.

At the time of the discovery of these tables the development of the theory of universal gravitation was in its infancy; —the simple elements of the gravitating forces were very imperfectly known; and consequently the elements of the planetary motions at that time, when thus corrected for the effects of the gravitating forces during a period of nearly five thousand years, would very imperfectly represent the original elements. But during the present century the theories and elements of astronomy have been very greatly improved; and we are now in possession of the means of obtaining the variations of all the elements of the planetary motions, during all past ages, more perfectly than would have been possible for our predecessors. And if our elements of astronomy, as they increase in precision, represent more and more perfectly, the elements recorded in these ancient tables, the presumption seems legitimate that the recorded elements are founded on the observed state of the heavens at that epoch. But if, on the other hand, they differ more and more widely from the recorded elements, the conclusion seems alike inevitable that they are not founded on fact, but are merely the idle fancies of the people by whom they are entertained.

We will now apply the test we have indicated to two of the astronomical elements which are given in the *Tirvalore Tables*, as they are technically called. These elements are the equation of the sun's centre (which depends on the eccentricity of the earth's orbit), and the obliquity of the ecliptic. We have chosen these two elements for comparison, because they may be readily determined by observation, and are alike independent of all knowledge of the true theory of astronomy. According to these tables, the epoch of which is assumed to be 4 952 years before the year 1850, the equation of the sun's centre was 2° 10′ 32″. And a writer in the *Encyclopedia Brittanica*, states that the equation of the sun's centre, at that epoch, should be 2° 6′ 28″, according to the formula of La Grange. This differs from the recorded value by only 4′ 4″. LaPlace's formula would diminish this difference by 30″. But according to my determination of the eccentricity, which is based on the latest values of the masses of the planets, the value of the equation at that time was 2° 7′ 24″, which differs from the recorded value by only 3′ 8″. According to the *tables* the obliquity of the ecliptic at the same epoch was 24°. LaGrange's formula gives 23° 51′ 13″, which differs from the recorded value by only 8′ 47″. LaPlace's formula gives 24° 5′ 50″ for the obliquity, which exceeds the recorded value by only 5′ 50″. And my own computation gives the obliquity at that time equal to 24° 3′ 8″, which exceeds the recorded value by only 3′ 8″.

According to this comparison the *tables* endure the test which we proposed to apply; and we may therefore regard the question of their authenticity as still an open one. I hope to be able, at an early day, to extend the comparison to the various other astronomical elements; and should the evidence continue to be cumulative, the conclusion will be inevitable that they were founded on observation.

The chart of the eccentricity of the earth's orbit may serve to give a general idea of the variations of the eccentricities of the orbits of *Venus* and *Mars*, although the latter would be subject to greater variations. A simple inspection of the chart shows that the average time required for the eccentricity of the earth's orbit to pass from a maximum to a minimum value, is only 45 500 years. This time seems to be very nearly independent of the amount of change required, in order to obtain these conditions.

If we now pass to the consideration of the elements of the four largest planets of the system, *Jupiter*, *Saturn*, *Uranus* and *Neptune*, we shall notice some curious and interesting relations. These planets compose a system by themselves, which is practically independent of the other planets of the solar system.

The maximum and minimum limits of eccentricity of these four planets are as follows:—

	Maximum.	Minimum.
Jupiter,	0.0608274,	0.0254942.
Saturn,	0.0843298,	0.0123710.
Uranus,	0.0779496,	0.0117464.
Neptune,	0.0145058,	0.0055729.

The whole limit of variation of eccentricity in *Neptune's* orbit is therefore only about one-half the present value of the eccentricity of the earth's orbit.

But the most curious relation developed by my researches, pertains to the relative motions and positions of the perihelia of the different planets. The mean motion of *Jupiter's* perihelion is exactly equal to the mean motion of the perihelion of *Uranus;* and their mean longitudes differ by exactly 180°. The mean motion of *Saturn's* perihelion is very nearly six times that of *Jupiter* and *Uranus;* and this latter quantity is very nearly six times that of *Neptune;* or, more exactly, 985 times the mean motion of *Jupiter's* perhelion is equal to 163 times that of *Saturn;* and 440 times the mean motion of *Neptune's* perihelion is equal to 73 times that of *Jupiter* and *Uranus.* The mean annual motions of the perihelia of these four planets are as follows; namely,

Jupiter and Uranus, ...	3″.716 923
Saturn, ...	22″.460 985
Neptune, ...	0″.616 686

The perihelion of *Saturn*, therefore, performs a complete revolution in the heavens in 57 700 years; that of *Jupiter* and *Uranus* in 348 676 years; while that of *Neptune* requires no less than 2 101 555 years to perform the circuit of the heavens.

We may observe that the law which controls the motions and positions of the perihelia of the orbits of *Jupiter* and *Uranus*, is of the utmost importance in relation to their mutual perturbations of *Saturn's* orbit. For in the existing arrangement the orbit of *Saturn* is affected only by the *difference* of the perturbations of *Jupiter* and *Uranus;* whereas, if the mean places of the perihelia of these two planets were the same, instead of differing by 180°, the orbit of *Saturn* would be affected by the sum of their disturbing forces. But notwithstanding this favoring condition, the elements of *Saturn's* orbit would be subject to very great perturbations from the superior action of *Jupiter*, were it not for the comparatively rapid motion of its perihelion; its equilibrium being maintained by the very act of perturbation. Indeed, the stability of *Saturn's* orbit depends almost entirely upon the rapidly varying positions of its transverse axis. For, if the motions of the perihelia of *Jupiter* and *Saturn* were very nearly the same, the action of *Jupiter* on the eccentricity of *Saturn's* orbit would be at its maximum value during very long periods of time, and thereby produce great and permanent changes in the value of that element. But in the existing conditions the rapid motion of *Saturn's* orbit prevents such an accumulation of perturbation, and any increase of eccentricity is soon changed into a corresponding diminution.

I said the mean angular distance between the perihelia of *Jupiter* and *Uranus* was exactly 180°. But the conditions of these motions are sufficiently elastic to allow of a considerable deviation on each side of their mean positions. In fact, the actual position of *Jupiter's* perihelion may be in advance of its mean place to the extent of 23° 54′; and it may fall behind its mean place to the extent of 22° 16′; while the perihelion of *Uranus*, may be in advance by 36° 26′, or behind its mean place to the extent of 45° 41′. It therefore follows that the nearest approach of the perihelia of these two planets will be 110° 25′, and their greatest distance apart will be 238° 42′. The amount by which these elements differ from the mean at present is only 21° 19′, or about one-third of their ultimate limit of deviation. This deviation from their mean places will continue to increase during a period of 35000 years, when it will attain the value of 68°. They will then alternately approach and recede from their mean positions, at intervals of time during a period of 170000 years, at which time they will be very near their mean positions,—the longitude of *Jupiter's* perihelion being 220° 14′, and that of *Uranus* being 36° 33′; and consequently the distance between them will be 176° 19′, which differs by only 3° 41′ from the mean value of that element.

The consideration of the phenomena depending on the variations of the nodes and inclinations will form a proper subject for another communication.

NOTE. In the accompanying Chart, the unit of abscissas is equivalent to 10000 years; and the unit of ordinates is equivalent to 0.001, for the Earth's orbit, and to 0.003 for Mercury's orbit. The curve, therefore, represents the eccentricity of Mercury's orbit only one-third of the actual value relatively to that of the earth.

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12. ON HANSEN'S THEORY OF THE PHYSICAL CONSTITUTION OF THE MOON. By Simon Newcomb, of Washington.

THE great reputation of the author has given extensive currency to the hypothesis put forth by Professor Hansen some years since, that the centre of gravity of the moon is considerably farther removed from us than the centre of figure. The consequences of this hypothesis are developed in an elaborate mathematical memoir to be found in the twenty-fourth volume of the Memoirs of the Royal Astronomical Society. But the reception of the doctrine seems to have been based rather on faith in its author, than on any critical examination of its logical foundation. Such an examination it is proposed to give it.

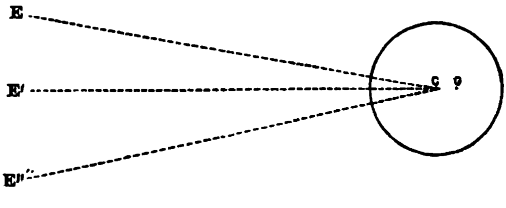
An indispensable preliminary to this examination is a clear understanding of what the basis of the doctrine is. Let us then consider these three propositions :

1. The moon revolves on her axis with a uniform motion equal to her mean motion around the earth.

2. Her motion around the earth is not uniform, but she is sometimes ahead of, and sometimes behind her mean place, owing both to the elliptic inequality of her motions and to perturbations.

3. Suppose her centre of gravity to be farther removed from us than her centre of figure, and so placed that when the moon is in her mean position in her orbit, the line joining these centres passes through the centre of the earth.

Let us also conceive that these two centres are visible to an observer on the earth. Then a consideration of the geometrical arrangements of the problem will make it clear that when the moon is ahead of her mean place the observer will see the two centres separated, the one nearest him being farther advanced in the orbit, while when the moon is behind her mean place the nearest centre will be behind the other. This apparent oscillation of the two centres is, indeed, an immediate effect of the moon's libration in longitude, as may be seen from the following figure in which the circle represents the moon, C and G her centres of figure and gravity, and E E′ E″ the



positions of an observer on the earth, relatively to the face of the moon, when she is behind, in, and ahead of her mean place.

Now the inequalities in the moon's motion computed from the theory of gravitation, are those of a supposed centre of gravity. But the inequalities given by observation are those of the centre of figure. Hence, in the case supposed, the inequalities of observation will be greater than those of theory. Also their ratio will be inversely as that of the distances of the centres which they represent.

Professor Hansen, in comparing his theory with observations, found that the theoretical inequalities would agree better with observation when multiplied by the constant factor 1.0001544. Supposing that this result could be accounted for on the hypothesis of a separation of the centres of gravity and figure, he thence inferred that the hypothesis was true. But the result cannot be entirely accounted for in this way, because the largest inequality of theory (evection) has a factor (eccentricity) which can only be determined from observation, and therefore, even the theoretical evection is that of the centre of figure and not of the centre of gravity. It must not be forgotten, that the eccentricity, which is not given by theory, is subject to be multiplied by the same factor that multiplies the other inequalities.

To be more explicit:—

Let e be the true eccentricity of the orbit described by the moon's centre of gravity; then the true evection in the same orbit will be

$$e \times A;$$

A being a factor depending principally on the mean motions of the sun and moon; and on Hansen's hypothesis the apparent evection, or that of the centre of figure, will be

$$e \times A \times 1.0001544.$$

On the same hypothesis, the eccentricity derived from observation, being half the coefficient of the principal term of the equation of the centre, will be

$$e \times 1.0001544,$$

and the theoretical evection computed with this eccentricity will be

$$e \times 1.0001544 \times A,$$

which is the same with that derived from observation. Hence:

The theoretical evection will agree with that of observation notwithstanding a separation of the centres of gravity and figure of the moon.

That Hansen overlooked this point is to be attributed to his method of determining the lunar perturbations, by numerical computation from the various elements of the moon's motion, so that the manner in which the inequality depends on the elements does not appear. It is only when we determine the perturbations in algebraic form that this dependence appears.

Passing now from the evection, the next great perturbation of the moon's motion is the variation. But the value of this perturbation has not been accurately determined from observation, because, attaining its maxima and minima in the moon's octants, it is complicated with the moon's semi-diameter, and parallactic inequality. Even if the semi-diameter is known, the two inequalities in question cannot be determined separately with precision, because their coefficients have the same sign in that part of the moon's orbit where nearly all the meridian observations are made. From this cause Airy's value of the parallactic inequality from all the Greenwich observations from 1750 to 1830 was 3″ in error. And when in his last investigation Airy rejected the observations previous to 1811, owing to some uncertainty as to what semi-diameter should be employed, the result was still a second too small. It is therefore interesting to find what value of the variation will result if we substitute the known value of the parallactic inequality in Airy's equations for the determination of that element. Neglecting those unknowns which have small coefficients, these equations are from 1806 to 1851,

1806—15	10.66 W	+ 28.14 V	= + 17.2
16—24	9.45	+ 30.92	+ 24.9
25—33	9.43	+ 29.26	+ 42.1
34—42	9.29	+ 27.28	+ 10.8
43—51	9.05	+ 23.36	+ 7.9
Sum, . .	47.88 W	+ 138.96 V	= + 102.9

In these equations W × 0″.73 represents the correction to the coefficient of variation, and V × 3″.77 that to the coefficient of parallactic inequality. We now know from recent special investigations that the latter coefficient is very near 125″.50. Airy's provisional one was 122″.10, whence

$$V = \frac{125''.50 - 122''.10}{3''.77} = 0.90$$

The sum of the preceding equations gives

$$W = 2.15 - 2.90\,V = -0.46.$$

The resulting correction to the provision variation (2370″.3) is therefore — 0.46 × 0″.73 = — 0″.34, making the variation

derived from observation	2369″.96,
while Hansen's theoretical value is . . .	2369 .86,
and Delaunay's	2369 .74.

The differences are too minute to found any theory upon. Leaving the evection and variation, the other inequalities are so minute that their product by Hansen's coefficient is altogether insensible.

Summing up the results of our inquiry, it appears that in the case of the evection, the supposed discordance between theory and observation would not follow from Hansen's hypothesis, and, therefore, even if it exists, cannot be attributed to that hypothesis as a cause. In the case of the variation no such discordance has been proved. In the case of the other inequalities the discordance would be insensible. The hypothesis is therefore without logical foundation.

The question whether the evection given by observation is really greater than that deduced from theory, although it does not affect our conclusion, is yet interesting and important. It appears from the commencement of Hansen's computations in his "*Darlegung*,"* that his theoretical perturbations were computed with an eccentricity equal to .05490079, and that, on comparing with observation, he found that this eccentricity should be increased to .05490807, which is the value adopted in the *Tables de La Lune*, and is greater than that employed in his theory in the ratio 1 : 1.0001326. If, now, he had employed this corrected eccentricity to recompute the evection, the latter would have been increased in the same ratio, and the outstanding discrepency between theory and observation would have been reduced .0001544 — .0001326 = .0000218 of it value, or 0″.10, a quantity no larger than may be attributed to the errors of theory and observation.

It does not, therefore, appear that there is any sensible discordance between the values of the two great perturbations of the moon which result from Hansen's theory, and those which result from all the Greenwich observations from 1811 to 1851.

*Erste Abhandlung, § 4, pp. 173-175.

13. THE RESUSCITATION OF THE CINCINNATI OBSERVATORY. By Cleveland Abbe, of Cincinnati, Ohio.

As our Association is so deeply interested in, and identified with, the progress of the scientific institutions of the country, I take pleasure in presenting to the members the following brief notice of what is contemplated, and being accomplished, in the city of Cincinnati.

The Queen City of the West, so prominent as an early patron of science and learning, has, through those who represent its wealth and cultivation, resolved to revive the interest in the Astronomical Observatory, founded by the labors of Professor O. M. Mitchel in 1842, to enlarge its field of usefulness and to establish its future existence upon a firm foundation.

It is well known that the expense of building and equipping the Cincinnati Observatory was borne by over a thousand individual contributors of small sums of money, the chief benefactor being the late Nicholas Longworth, Esq., the donor of the present site upon Mt. Adams. The countenance of our own Association, and especially the cordial encouragement and timely assistance vouchsafed for many years by the late lamented Superintendent of the Coast Survey, contributed not a little to the prosperity of the Institution. Never having received any permanent endowment, however, it inevitably followed that on the removal of General Mitchel from Cincinnati, the interests of the Observatory were generally lost sight of. Thus it has happened that for the past ten years the corroding effects of time have become more and more plainly visible, while on the other hand the rapid growth of the city has surrounded the Observatory site by dwellings and factories, whence issue clouds of smoky vapors even in the hottest months of summer.

At a meeting of the Trustees of the Observatory, held last winter, the interests of the establishment were entrusted to me as Director, and I take pleasure in bringing to the notice of this Association the broad and enlightened policy that the Trustees have decided to adopt as being for the best interests of the Institution.

"Usefulness" being the criterion by which business men very properly judge of the value of an Institution, we shall necessarily seek to advance not only science but also its practical applications. Accordingly it has been resolved that our complete ideal must embrace the field of activity indicated by the following articles:

1. In Astronomy, the application of large Equatoreals to the study of the heavens will enable us to further the science, properly so called. The establishment of a meridian transit and a vertical circle will hereafter follow, and will especially be necessary in connection with the Geodetic works to be mentioned.

2. In practical Astronomy, the determination of latitudes and longitudes, as the basis of accurate maps of the surrounding country, will claim a large share of our attention.

3. In connection with the latter, Hypsometry and Geodesy in its general expansion will be cultivated; combined herewith the conduct of a "Pendulum Survey" will be of great interest, especially if, as we expect to be able to do, the use of the Repsold Bessel Pendulum be introduced. Although an extended survey of the Ohio Valley will require a long series of years, yet it will ever be kept steadily in view as the appropriate work of the Observatory. The proposal to establish a special school of Geodesy, under the auspices of a University about to be inaugurated at Cincinnati, has met with a favorable consideration.

4. The Meteorology of our country is a matter of such vital importance to the national interests, and one so interesting to the astronomical observer, that it has been decided to establish a complete Meteorological Observatory, furnished with self-recording instruments, so soon as the proper funds are secured.

5. In direct connection with the latter it is considered desirable that there be published a daily meteorological bulletin, compiled from telegraphic dispatches from all parts of the Ohio Valley. For this, the central position of Cincinnati offers great advantages, and we shall expect to distribute this bulletin widely to those places where the knowledge it contains is desired.

6. It is considered that we shall not depart too widely from our province, if we include in our Institution the Magnetic Observatory, also; this will complete our equipment as an

Astronomical and Physical Observatory, and seems especially called for when we consider the fragmentary condition of our knowledge of the magnetic elements in the interior of this continent.

While the works here contemplated will demand a large force of men and instruments, they equally necessitate the removal of the Observatory to a new and more favorable location; this step having been at once decided upon it is our confident expectation that the removal will be effected within a year; therefore at the next meeting of the Association I hope to present a report of that which has been done, rather than of that which is contemplated.

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14. PHOSPHORIC ACID, A CONSTITUENT OF BUTTER. By Professor E. N. Horsford, of Cambridge.

LIEBREICH* found some three years ago in the alcoholic extract of the brain, and also of nerve fibre, a crystalline substance of fatty character which readily broke up into several fatty bodies, including cerebrin, stearic, and perhaps other fat acids, and a body having the composition $C_{10}\ H_{14}\ NO$, which Liebreich called neurine, which was combined with glycerophosphoric acid.

This nitrogenous body—*neurine*—was observed by Wurtz to consist of the same elements in the same proportions that would be required to form an oxide of ammonium, in which one of the atoms of hydrogen is replaced by ethyl, and the other three by three atoms of methyl, thus:

$$N \left\{ \begin{matrix} C_4\ H_5 \\ C_2\ H_3 \\ C_2\ H_3 \\ C_2\ H_3 \end{matrix} \right\} O = C_{10}\ H_{14}\ NO.$$

Bæyer and Liebreich found sugar in the compound, and the whole body yielded to analysis the constitution of

$$C_{232}\ H_{241}\ N_4\ O_{39}\ PO_5.$$

It is, possibly, the body formerly known as lecithin, and was called by Liebreich, *Protagon*.

* Liebig's Jahr's Bericht. 1865, p. 647.

Wurtz attempted the synthetic production of neurine, and succeeded perfectly so that in all its reactions the body he obtained corresponded with the neurine derived from the brain.

It combines with platinum and gold salts and with various acids yielding the same crystalline forms and the same reactions that were obtained with the neurine procured from the brain.

Since then, Hermann has found protagon in the blood corpuscles, and Hoppe-Seyler in the serum, and in the red and white corpuscles, in Indian corn, in wheat, in wine ferment, in the yellow of the egg, and in some fat oils.

In view of these facts it seemed highly probable that it would be found in butter, which is the fat constituent of milk, from which the blood and brain of the young mammals derive their nutriment.

In my first experiment in search of this body, or of the phosphoric acid it contains, I selected a very choice sample of butter, quite free from buttermilk and water, rubbed it up with carbonate of magnesia and pulverized caustic potassa. It saponified in a short time to a mealy mass. This mealy mass I threw, in successive small portions, into a platinum crucible and burned. The residuum I extracted with nitric acid and water, filtered off the unburned charcoal, and tested with molybdate of ammonia. The precipitate was so immediate and large, that I was led to suspect that either my potassa or magnesia contained phosphoric acid. They proved to be entirely free from it. It then occurred to me that a trace of caseine or of buttermilk present in the butter might be the source of the phosphoric acid.

I proceeded then to treat a mass of butter with ether, pouring off the extract when the residue was reduced to a pale milk-and-water fluid. The extract I left to spontaneous evaporation. The milk-and-water fluid yielded scarcely a trace of phosphoric acid to molybdate of ammonia.

From the ether extract on slight concentration, there separated abundant crystals, and at length the whole became, as the ether evaporated, a semi-fluid crystalline magma.

This mass I rubbed up with carbonate of magnesia and powdered hydrate of potassa, calcined as before, and dissolved in diluted nitric acid, filtered through sand previously boiled with hydrochloric acid and thoroughly washed with water, and tested with molybdate of ammonia. The yellow precipitate was prompt and abundant.

There can, therefore, be no doubt of the presence of phosphoric acid in the etherial solution of this sample of butter. It is not improbable that it is present in all butter.

As butter has been found to contain:—

Butyric acid,	$C_8\ H_8\ O_4$	Palmitic acid,	$C_{32}\ H_{32}\ O_4$
Caproic "	$C_{12}\ H_{12}\ O_4$	Margaric "	$C_{34}\ H_{34}\ O_4$
Caprylic "	$C_{16}\ H_{16}\ O_4$	Stearic "	$C_{36}\ H_{36}\ O_4$
Capric "	$C_{20}\ H_{20}\ O_4$	Butic "	$C_{40}\ H_{40}\ O_4$
Myristic "	$C_{28}\ H_{28}\ O_4$		

glycerine and phosphoric acid, and, as Protagon has been found to contain glycerine, sugar, phosphoric acid, margaric acid, stearic and other fat acids, it is hazarding little to say that protagon or some nearly allied body will probably be found to be an important constituent of butter.

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15. Fluorine in the Human Brain. By Professor E. N. Horsford, of Cambridge.

The large percentage of phosphoric acid in the brain and nerves, amounting (according to v. Bibra, Bourgoyne, Fremy and Warren,*) to some 4.50 per cent. in the dried brain, suggests the possibility of finding fluorine in the same tissues, since fluorine is a very frequent concomitant of phosphoric acid in the mineral kingdom, and is found as fluoride of calcium in the teeth and bones. It might, moreover, very naturally be expected to be found in the brain, after it was found in the blood by George Wilson in 1846, and since then by Nicklés in albumen, gelatine, hair, blood and urine.

The specimen of brain which I employed in my analysis was one long kept in alcohol, but from which, through neglect, all the alcohol had evaporated, and there remained a compact, corrugated mass, of a texture somewhat like that of a moderately hard pine-apple cheese. Pieces of this mass, ground up

*From unpublished manuscript notes supplied to the author by Professor C. M. Warren.

with caustic potassa or lime, gave readily the indications of phosphoric acid, with nitric acid and molybdate of ammonia. The same reagents indicated phosphoric acid after digestion of portions of the brain with oil of vitriol.

The chief difficulty in obtaining trustworthy evidence of the presence of fluorine lies in the circumstance that the reagents employed in the reaction may sometimes, indeed do oftentimes, contain fluorine.

My lime was prepared by carefully elutriating water-slacked quick lime, containing considerable silicate of lime and a trace of phosphoric acid, and subsequent ignition of the dried deposit. This gave a substance of extreme fineness. Silica was prepared from common water glass, by separation with hydrochloric acid, long continued washing and drying. These two ingredients, with oil of vitriol from my laboratory, mixed with silica, in all respects as in the last experiment, except that the brain was not present, gave no indication of fluorine.

With these reagents I proceeded to test the dried brain for fluorine. A portion of it was ground up with the dried lime and calcined on platinum, the hydrogen and carbonaceous matter burning off with great facility.

The calcined mass was then ground with silica, prepared from the hydrate and mixed with the oil of vitriol in the bottom of a test tube. A tube, moistened in the interior, was inserted through a cork into the test tube, and the test tube and its mixed brain ash, lime, silica and sulphuric acid, gently heated. The fumes that arose condensed in part on the upper portions of the test tube, and along the moistened interior of the inserted tube, forming a white film which was non-volatile before the blow-pipe. This was *silica* derived from the *hydro-fluo-silicic* acid, evolved from the mixture, and proof of the presence of *fluorine*.

Another sample of the brain was rubbed up with powdered hydrate of potassa and calcined magnesia to a paste calcined in platinum, saturated with water glass, again heated to drive off the water, powdered and mixed with oil of vitriol in a flask. The vertical eduction tube was of nearly half an inch calibre and four inches high, and connected with a smaller curved tube, which discharged under water. On applying heat to the flask, there appeared a white powder along the moistened eduction pipe and the curved tube, and finally in the water, which latter readily dissolved in potassa liquor. The water glass, magnesia and potassa, as well as the sulphuric acid, were proved to be free from fluorine. This experiment was conclusive as to the presence of fluorine in the brain.

A similar experiment with the residue obtained from treating a portion of the brain for a series of days with successive portions of nitric acid and evaporating to dryness yielded a similar result, though, for some reason, not so marked as that obtained with the ash by burning the quick lime mixture, and also by the process with water glass.

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16. On the Source of Free Hydrochloric Acid in the Gastric Juice. By Professor E. N. Horsford, of Cambridge.

The long disputed position of Prout, that the gastric juice contains free hydrochloric acid, was at length established by C. Schmidt, who in an absolute quantitative analysis of the juice found about twice as much hydrochloric acid as was required to neutralize all the bases present. The prolonged discussion of this subject now since 1823, has brought to light through the researches of Lassaigne, Tiedemann and Gmelin, Berzelius, Lehmann, Claude, Bernard, Blondlot and numerous others, the unmistakable evidence of the presence of lactic acid, and of acid-phosphates in the gastric juice, which might or might not be due to the presence of lactic acid or hydrochloric acid. A point of special interest to the chemist and physiologist, still remained, and was this:

How could free hydrochloric acid be secreted from the blood which is an alkaline fluid? This question was submitted to experiment with entirely satisfactory results.

The blood freshly drawn consists of a fluid (the plasma) in which there are myriads of exceedingly minute irregularly spherical bodies (the corpuscles) swimming about. The *plasma* consists of two bodies, one of which, the fibrine, spontaneously separates from the other, the serum. The *corpuscles* are little sacs of delicate animal membrane enclosing a fluid. This fluid has an acid reaction and its ash contains a monobasic alkaline phosphate. The fibrine of the plasma contains a monobasic phosphate of lime, though the plasma as a whole has an alkaline reaction, and its ash contains a great excess (11 per cent.) of chloride of sodium (common salt). The moist corpuscles constitute about one-half of the blood.

The relations of the fluid within the corpuscles to the plasma which surrounds them, is the same as that of the yolk to the white of the egg, and the ratio of the fixed base to phosphoric acid is alike in both cases.

1 : 1 in blood corpuscles, | 1 : 1 in egg yellow.*

The ash of albumen contains nearly one-third of its phosphoric acid, as monobasic phosphate (3.79 : 1.15) according to Poleck, while the same analyst gives for the analysis of the yellow 66.70 per cent. of phosphoric acid, of which 41.33 per cent. is free.

Under pressure the fluid contents of the corpuscles pass through their membranous walls, and through the walls of the nutritive capillaries. Such pressure exists when the blood vessels of a particular organ are engorged, as the blood vessels of the stomach always are in healthy digestion. Engorgement is the equivalent of obstruction. Under the pressure that follows, the fluid contents of the corpuscles will pass through their membranous walls and mingling with the relatively lessened plasma, pass on through the walls of the capillaries. It is obvious that the acid character of this mixed fluid is simply a question of the relative amount of acid fluid pressed from the corpuscles, and of alkaline plasma which the expressed fluid finds. Under such pressure the areolar tissue under the mucous membrane is charged with the mixed fluids of the corpuscles and plasma. This mixture contains, therefore, acid phosphates and chloride of sodium.

The mucous membrane of the stomach presents on its under surface the mouths of numerous microscopic tubes, which like a stocking are sometimes single blind sacs, or like a glove terminate in several blind sacks, like the glove fingers. In the bottoms of the tubes and along their sides are several closed spherical sacs, containing other lesser sacs and fluid within.

*Liebig's Jahr's Bericht. 1850, p. 559.

The tubes as a whole dip down into the spongy tissue that underlies the mucous membrane, where they are surrounded by the fluid poured from the surrounding net-work of nutritive capillaries, which fluid contains acid phosphates and chlorides.

Now by pressure and osmosis a portion of this fluid will pass through the walls of the gastric tubes, and the question is whether the fluid that goes through will contain free hydrochloric acid.

The experiments I have made are conclusive on this point.

By employing acid, phosphate of lime and common salt, I had this advantage, that as increased acidity on the one hand is a just inference from increased alkalinity on the other, and as increased alkalinity would be shown by the precipitation of phosphate of lime — a visible white powder — I could determine the qualitative fact without the difficulties and delay attendant on accurate quantitative analysis of the solutions on both sides of the membrane before and after the experiment.

With acid phosphates of lime in my earlier experiments I was embarrassed with the presence of sulphate of lime in the powder, so that what was at first supposed to be pure phosphate of lime, was found to be in part sulphate of lime. This sulphate was due to imperfectly washed parchment paper employed as a dialyser. This difficulty overcome the experiments were made with parchment paper prepared from German and Swedish filter paper, as well as with gold-beater's skin.

I employed acid phosphate of lime, successively, with chloride of sodium; chloride of potassium; chloride of ammonium; chloride of magnesium; chloride of calcium; with all of which there was the same kind of evidence of increased alkalinity on one side, and of course corresponding increased relative acidity on the other. The same effect took place with acid phosphate of soda and chloride of calcium.

It follows then from what we know of the composition of the blood, its condition in the walls of the stomach, and the structure of the gastric tubules that free or uncombined hydrochloric acid must find its way into the sacs at the bottom of the tubules. It is of course mixed with acid phosphates and alkaline chlorides. The sacs at the bottom of the tubules, by a secondary dialysis, concentrate the acid solution. Swelling by endosmosis, and corroded by the acid juice, at length they burst, and the liquid contents, together with the disintegrated and partially digested membrane of the sacs, pass out to the stomach to constitute the gastric juice, the free hydrochloric acid and the disintegrated tissue (the pepsine?), to act in the liquefaction of the food.

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17. ON THE ACTION OF LIGHT UPON THE BROMIDE AND IODIDE OF SILVER. By M. Carey Lea, of Philadelphia, Pa.

THE object of the following paper will be to contribute the result of some new experiments which, when taken together with observations already made, may add something to our knowledge of this very difficult subject.

It is an already ascertained and accepted fact that when bromide of silver is exposed to light, it undergoes decomposition with elimination of either bromine, or a bromine-compound, whilst it is itself reduced to a sub-bromide. This reduction is accompanied with a very distinct darkening. I have myself recently repeated this experiment under conditions of which I sometime back laid down as the basis for accurate investigation in this direction, viz., that thin films of the silver haloid, whether bromide or iodide, should be formed to the absolute exclusion of all organic matter. This I effected by silvering a plate of coarsely ground glass with specular metallic silver. Its film was then converted into bromide or iodide as might be desired, by plunging, in the one case into a bath of strong bromine water, in the other, into a solution of iodine in iodide of potassium. These plates when thoroughly washed, afforded films of the pure silver compound in a condition as convenient for experiment as if the compound had been supported on paper, collodion or other basis, and free from the fatal objection of the presence of organic matter, an objection which has already vitiated so many laborious examinations in this field. In the present case, it was found that such a film of bromide of silver by long exposure under a design cut out of an opaque object, gave a strongly marked image. The exposure required is a long one: — fifteen minutes of bright sunshine yielded no result, but four hours gave a distinct impression — this is when the bromide is pure and isolated.

It is also an established fact that no such result can be obtained with *pure* iodide of silver likewise isolated. The most careful investigators, amongst whom I may cite Dr. Vogel of Berlin, have failed to detect any elimination of iodine, nor is a visible image obtained by an exposure of equal length to the foregoing.

These premises, correct in themselves, have led to certain erroneous conclusions, as I shall endeavor presently to show.

It is a well established fact that an exceedingly brief exposure to light, will, under favorable circumstances, produce a *latent image* on either iodide or bromide of silver; this image, invisible at first, is brought out by the application of suitable developers.

Now if we admit that iodide of silver is incapable of chemical decomposition by the sun's rays, we are forced at once to the conclusion that the latent image impressed upon it by light, is purely physical in its nature, since there is obviously no other alternative. This question is one which I have devoted much study to, and recorded results elsewhere strongly supporting the physical theory. It is not my intention to discuss that part of the subject here, but to examine the state of the question, more especially as regards the bromide of silver.

As bromide of silver is well known to undergo a distinct chemical decomposition by the action of light, it has never seemed necessary to apply to it the hypothesis of the formation of a physical image. It seemed probably more natural to suppose that, as a prolonged exposure to light produced a visible image, so a very brief one produced an image, which, though invisible, was of precisely the same nature as the visible image; that is, an image which though invisible from its excessive thinness and delicacy, yet formed the nucleus which by development became the visible image. This view was thoroughly consistent with the common analogies presented by photographic operations, in which, by the continued action of developers, an image though exceedingly thin at first, was gradually brought out to any desired strength. It will be my object here to show that this view, plausible as it is, is wholly incorrect, and that bromide of silver under suitable conditions is capable of forming a latent image in which chemical decomposition plays no part, and which, therefore, must be considered as a mere molecular alteration, a physical, as contradistinguished from a chemical image.

But this physical image on bromide of silver is extremely and remarkably different from that found upon iodide of silver. The following statements will place the difference in a strong light.

1. The physical image is formed on iodide of silver isolated from all other bodies.

2. That on bromide is found, so far as my experiments have gone, *only in the presence of organic matter.*

3. The physical image on iodide of silver can be called forth in the presence of silver or of some other metallic body* only.

4. The physical image on bromide of silver can be developed in the complete absence of any metallic body.

These four distinctive positions are based upon a very careful series of experiments made by myself. The third of these positions, viz., that the invisible image upon pure iodide of silver isolated from all other bodies can be evoked in the presence of nitrate of silver only, has, I believe, never before been made,† and it is contrary to views which I have myself before entertained and expressed, which I avail myself of this opportunity to correct.

I proceed to the proof that a true physical image, apart from chemical decomposition, may be formed upon bromide of silver. This, as I have already said, takes place in the presence of an organic body.

Let a collodion containing bromide of silver and free nitrate of silver be extended on glass. Let this after setting be washed under a stream of water, and then be plunged into a solution of tannin, and dried. We have here a sensitive dry plate. Let this be exposed for a brief time in the camera. A latent image is formed. Let us now plunge the plate into a solution of pyrogallic acid; the image appears.

* I have shown elsewhere that these images may be developed with proto-nitrate of mercury.

† Until the experiments were made upon which this paper is based, it had never been ascertained whether the alkaline development in the absence of silver, so effectual with bromide of silver, was possible upon pure iodide. These experiments decided the question in the negative.

In the ordinary development of a dry plate, the above may or may not, form a part of the whole operation. Let us here separate it from any other operations and consider it apart.

What can have caused the latent image to appear? It could not have been that an infinitesimal *chemical* image of sub-bromide of silver acting as a nucleus, was brought up by the action of the developer to a visible intensity, because pyrogallic acid by itself has no power to do this, free nitrate of silver must be present, and in this case, none was so, as it had been removed by washing.

Thus, as we are absolutely excluded from this explanation, we find but one alternative, namely, that that portion of the film upon which the light had acted, was so modified thereby, that it was brought into a condition to be more easily decomposed by pyrogallic acid than the portion which had not been acted upon. We have just seen that we are excluded from the supposition that the modification was chemical; it must therefore have been molecular or physical.

Perhaps this may be rendered clearer by the following argument.

Bromide of silver is almost colorless; sub-bromide is deep blackish brown in color.

At the beginning of the operation, namely, before the exposure, the film was colorless.

At the end of it, when the image has been evoked, that image consists of deeply colored sub-bromide.

Now the coloration takes place only after the application of the pyrogallic acid, and it is evident that the *chemical* decomposition is simultaneous with the coloration; consequently the modification which took place during the exposure was not chemical, but purely physical.

It is unquestionably true that a prolonged exposure will produce a visible image independently of development; not an exposure of the length previously spoken of, measured by hours of bright light, for there the bromide was isolated, and here it is supposed to be in contact with more than one organic substance, but a comparatively short one in the camera may produce traces of an image. But the complication here is only a seeming one.

If it were necessary in the case of a bromide film (as it is in that of an iodide) that free nitrate of silver should be present, it then might be alleged that the strong visible image had been built up upon a thin invisible one. But this is not at all so. Admit in the case of a bromide film that a visible image *is* present, and let us argue on such a case.

The production of a visible image unquestionably indicates (so far as that image is concerned) chemical decomposition. But when we apply pyrogallic acid, we do not build up on that image, for silver in solution must be present for that. There being none present, there is no possible way in which the strong visible image can come out, except by the decomposition of portions of bromide of silver, not previously decomposed, and this by the agency of the pyrogallic acid.

If then this be so, if portions of the bromide film not decomposed by light, but simply acted upon by it, are subsequently decomposed by the action of pyrogallic acid, whilst those portions of the film not influenced by light, are not decomposed by the pyrogallic acid, then it follows that the action of the light was so far simply physical.

On such a plate, then, when exposed to light, two actions manifest themselves. There is a slow chemical action, which if it be continued sufficiently long, may be carried to any extent. But there is also a much more rapid molecular change, so that before the chemical image is visible, there has been formed a physical image capable of development by pyrogallic acid. And after a visible chemical image has been produced by prolonged exposure, there is simultaneously present a physical image which is capable of development by agents which have no action whatever upon the chemical image also present, and can neither add to nor take from the latter's intensity, but which bring out the previously physical image into a strength far greater than that of the previously visible image.

In the foregoing explanation I have carefully placed aside the *alkaline* method of development, because if I had argued upon it, it might have been alleged that the carbonate of ammonium employed (for example) dissolved bromide of silver from the film and so afforded a soluble silver salt towards building up a visible image upon an invisible *chemical* nucleus. To avoid this objection I have selected a case in which it cannot be alleged that any trace of a soluble silver salt is present. We are thus thrown back, for the production of the developed image, upon the bromide of silver in the film, and thus we distinctly recognize the fact that the bromide of silver has, by the influence of the light acquired a property which it had not before, namely, a greater facility for decomposition by pyrogallic acid. As the act of this acquisition was attended by no chemical decomposition (which set in only when the pyrogallic acid was applied) it must, I think, be admitted that I have demonstrated that bromide of silver may, under favorable circumstances, receive a physical or purely molecular impression from light, leading to, but in itself distinct from, chemical alteration.

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18. The Application of Carbonic Acid Gas in the Extinguishment of Fire. By E. L. Buttrick, of Chicago.

The powerful effect of carbonic acid gas in checking combustion has long been known. Since the time of Sir Humphrey Davy, who was one of the first to describe its properties and portray its effects as a fire subduing agency, a vast amount of scientific research and mechanical ingenuity have been expended, in order to render it available in the service of man, to stay the progress of that destructive element, which at times defying all human control and all the simpler antagonisms of nature, destroys the proudest monuments of art, and sweeps away in an hour the accumulated results of centuries of toil.

During the past sixty years, both in England and on the continent of Europe, there have been numerous contrivances, many of them quite ingenious in their mechanical details, to accomplish that result, which when arrived at, it was known from the chemical properties of the agency employed, would give to man a more complete and immediate control of the destructive element of fire, and render it quite impossible for any extensive conflagrations to occur in the future, like those which in the past have swept away the substance of thousands of people, and blighted the prosperity of towns and cities.

But until recently scientific research and mechanical skill have failed in all their endeavors to utilize the gas for this purpose to any considerable extent. Indeed, so elaborate had been the plans, so numerous the contrivances, and so frequent and complete the failures, that the author of a recent able and comprehensive English treatise upon the subject of extinguishing fires, expresses the opinion that it is useless to push such investigations farther, as in the very nature of things it is not possible by any mechanical contrivance, to render the use of carbonic acid gas at all available as an adjunct of the fire department, and a co-worker with man in protecting life and property from the perils of fire.

How ill grounded were the conclusions of the author referred to, the progress of events during the last few months has effectually demonstrated, for in various towns and cities of the United States, from Portland in Maine, to Milwaukie in Wisconsin, the fact has been demonstrated to thousands of astonished and admiring spectators; and as we are informed without a word of cavil, either from men of science or those unlearned in its mysteries, that the machinery was really invented; cheap, simple and powerful, capable of producing carbonic acid gas instantaneously and constantly, to an unlimited extent, which when so produced could be directed upon any part of a burning building by the same means, with the same precision, and with twenty times the effect that a stream of water could be directed from such engines as are now in common use.

Thus it seems to have been reserved for our own country to demonstrate that the inventive genius of her citizens which achieved for her such renown in the telegraph, the reaper and the sewing machine, was still unexhausted, and capable of winning new triumphs even where other nationalities had totally failed.

Before describing the invention to which it is the purpose of this paper to call your attention, it may be proper to give a brief sketch of other inventions for this same object, which take precedence in point of time, and have attracted considerable attention in both hemispheres.

The first is a claim for an invention for utilizing carbonic acid gas in the extinguishment of fire by A. Vignon, of London, who procured a patent in the United States in 1862. His claim consists of the employment of a solution of carbonic acid gas in water, either at high or low pressure, for extinguishing fires on land or on board vessels, and the construction and employment of apparatus either portable or fixed, for extinguishing fires in which a solution of carbonic acid gas in water is prepared and stored up, and whence such solution is ejected with the requisite force without the aid of pumps. But Vignon describes in his claim no apparatus by which a solution of carbonic acid gas in water may be made available, and there is no evidence that any apparatus constructed by him for that purpose ever came into practical use. After this an attempt was made to apply free carbonic acid gas to the extinguishment of fire, by means of what is known as Phillip's Annihilator, but the invention proved impracticable, from the fact that the gas could not be directed with certainty upon any given point. The next attempt to utilize this gas for the purpose mentioned was more successful. It is an English invention, and is used to a very great extent in Europe, where it is known as Seavey's Fire Extinguisher, and in this country as Barogwanath and Van Wiskers' Improved Fire Annihilator or Portable Fire Extinguisher. It consists of a cylinder, in which by an ingenious contrivance the bi-carbonate of soda and acids are confined in solution, by which carbonate acid gas is generated in large quantities. To the cylinder is attached a faucet connected with a short hose. When the faucet is opened the pressure of the gas is such that the contents of the cylinder are thrown out with considerable force. These cylinders hold from three to eight gallons, and are designed to be carried on the back by persons using them at a fire. This little invention is of great value in extinguishing incipient fires, and is very extensively used in manufactories and large public buildings. It is entirely practical in its results, and so far as its power extends accomplishes the object for which it is designed. But something more was needed than the construction of a simple machine for the production of a carbonic acid gas to quench fire in the incipient stages of combustion; and possessing more power than could be derived from the expansive force of the gas generated by the acidulous and alkaline solutions, and that want has at last been supplied. The apparatus consists of an engine in the ordinary form of a hand fire engine, except that the "tub" forming the body of the engine is divided into two compartments, in each of which is a force-pump of the usual form of construction, connected by proper metallic pipes and valves with a metallic air chamber firmly attached to the platform upon which the tub rests on a level with its bottom, and located between the two compartments. To the air chamber a hose is attached for the conveyance of the stream to any point desired, as in the case of an ordinary fire engine. In case of fire the separate compartments are supplied with water, and into each of them is thrown such acid and alkaline preparation as will best subserve the purpose of generating carbonic acid gas, having always in view economy of material in comparison with the results to be attained. The acid and alkaline solutions are inert or quiescent until they are brought together in the air chamber, and this is done by means of the pumps, which, worked in the usual manner, force at every stroke the contents of each compartment together. The effervescence is then instantaneous and complete, and the gas is evolved with such rapidity as to produce a pressure upon the air chamber equal to sixty pounds to the square inch, which can be maintained continuously, so long as the pumps are worked and the tanks supplied with water and the necessary chemicals. To the expansive force of the gas produced in the close air chamber, is added the additional arbitrary power of the pumps, whatever it may be, dependent upon the relative size of the cylinders to the power exerted at the brakes. It has been demonstrated by repeated experiments, that a small hand engine not larger than those used for garden purposes, and worked by two men, can throw a stream of carbonic acid gas and water combined, through a three-quarter inch hose and quarter-inch aperture at the pipe, that is more effective in subduing a conflagration than a stream of simple water thrown by the ordinary hand fire engine, when worked by the united power of forty or fifty men.

It is quite unnecessary for me, in such a presence, to detail the manifest advantages of this simple mechanical contrivance, whereby a well known scientific principle, the absolute certainty of which has long been known and demonstrated, is thus made applicable to useful ends, and rendered subservient to the welfare of man in the protection and preservation of property and life. Your own studies have led you into such investigations as will enable you to appreciate, far better than any suggestions of mine can aid you to comprehend, the vast and beneficent results to our country and mankind which are to flow from the increased power it gives to control that fiery element, which when under proper restraint is always the friend and servant of mankind, but once his master proves the enemy and destroyer of his property and his hopes.

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19. On the Combining Power of Chemical Elements. By Samuel D. Tillman, of New York.

Since the promulgation of the doctrine of types and substitutions, the saturating powers of chemical bodies have been quite satisfactorily represented by symbols. The prominence which the subject of atomicity has recently assumed is partly the result of the plain and simple manner of illustrating it. By means of the new notation and several additional characters or signs, an ideal structure of a chemical body is represented; for of the real structure nothing is known. In it the saturating power of an atom or a combination of atoms is measured by degrees, the lowest power being taken as the unit of measurement. A given number of combining units require a like number of other combining units to complete the structure. The new body is always the result of duality, and its form or type will be retained while its chemical functions may be entirely changed by displacement; for certain parts of the structure may be removed provided a substitution of the same equivalence is made to preserve the molecular equilibrium. Thus the chemist assigns substitution-values to every simple and complex radical, and designating them by numbers, from one up to six, is prepared to combine his symbols in a process of matching, which, although quite as simple as that with dominoes, affords the highest satisfaction, because it is always associated with the order, rapidity and precision of molecular changes. Yet this kind of chemical reckoning has one serious drawback, namely, the atomicity of the same element is not an invariable value. It is often decreased by a duplication of similar atoms, and in some remarkable instances among monads, it seems to be increased beyond its normal energy by a kind of induction which has not yet been accounted for. Saturating power depends on causes and conditions still wrapt in obscurity. Equivalence involves higher questions than those of quantity and quality, as these terms are applied to ponderable matter, for two elements having diverse chemical functions and widely-differing atomic weights, like hydrogen and chlorine, sometimes assume the same relative position in the mazes of chemical combination. In many instances attractive force or affinity seems subservient to fitness of place; hence has arisen the distinction of chemical and mechanical or molecular types, which can only imply that one kind of force is more effective than another in completing what may be termed the symmetry of the chemical structure. The smallest quantity of an element which can enter into combination has an invariable weight, and doubtless a normal sphere of attraction, which may be modified in its relations to that of another element, on changing the rate and range of the undulations of the inter-atomic medium through the agency of light or heat; or on disturbing its general equilibrium by what is termed electric force. It is evident that the minute changes which a molecule undergoes in chemical action involves a problem of motions too complex for complete solution.

Considerable diversity of opinion still exists among advanced chemists both as to the best form of types and the highest saturating power of several elements. Indeed the value of the same element varies so much under different conditions, that some writers, regarded as authorities, are now inclined to include all simple bodies in two classes, one embracing combining units expressed by even numbers, and the other those expressed by odd numbers. Some of the late conclusions of the ablest advocates of the new views would seem to obliterate even this single dividing line of distinction. Professor Wurtz of the College of France, in his admirable lectures on "Types and Atomicity," is reported by the London *Chemical News* as saying: "Nitrogen seems to us diatomic in the binoxide of nitrogen." This body according to the new notation is a combination of one atom of nitrogen (14) with one of oxygen (16). The statement taken literally would place nitrogen in both classes alluded to. With equal consistency it may be asserted that nitrogen in the peroxide of nitrogen is tetratomic; and by a parity of reasoning nitrogen becomes a Proteus among types! Now, while it is admitted that only two-thirds of the normal saturating power of an atom of nitrogen is expended in combining with one of oxygen, it may with great plausibility be assumed that the remaining unexpended combining unit actually determines the value of this body as a monad radical. It cannot exist in a separate state, but two such compound atoms or atomoids will unite like two atoms of nitrogen, to form a molecule. In this manner the peroxide of nitrogen is shown to be also monatomic, because two atoms of the dyad oxygen have a substitution value of four, and exceed by one that of nitrogen. In one case the value of the radical is expressed by $3 - 2$, and in the other by $4 - 3$. Nitrous anhydride and nitric anhydride are formed on the water-type by the addition of an atom of oxygen to two of these radicals respectively. The lowest oxide of nitrogen, laughing gas, is an inert body having neither alkaline nor acid characteristics. Its atomoid contains two atoms of nitrogen and one of oxygen, and its combining power is expressed by $2 \times 3 - 2$. Should it hereafter be found to act as a radical it is here predicted that its value will be tetratomic.

An example bearing still more directly on the question of the atomicity of nitrogen is that of cyanogen, an atomoid of which contains an atom of carbon (12) and an atom of nitrogen (14). If these two atoms meet on equal terms, either the carbon atom becomes a triad, or the nitrogen atom a tetrad. On the other hand if it is assumed that both elements retain their normal saturating power, carbon as a tetrad and nitrogen as a triad we satisfactorily account for its monatomic substitution-value which is expressed by $4 - 3$. In its general behavior this radical resembles the halogens; thus presenting an apparent anomaly in the formation of a compound having powerful electro-negative characteristics found in neither of its constituents, and proving that the predominence of atomicity in an element does not necessarily impart to the body of which it forms a part any of its chemical functions; in other words, that the atom of lowest combining value is not merged in that of the highest, but that, as between the two, the merging is mutual; the result being an absolutely new body bearing no resemblance to its constituents in a separate state, and exhibiting only that relation to them which arises from the law of differential atomicity. This law was expressed in a paper read by me before this Association in 1866, as follows: "The atomicity of a torso (that is an imperfect body having no separate existence) or of a radical containing one atom of an element united to one or more atoms of another element is equal to the difference between the normal saturating power of its components."

It is difficult to trace all the steps of the resulting atomicity in bodies containing only two elements when the atoms of both elements are duplicated, because such duplication involves an unequal decrease in their normal atomicity. However, in a homologous hydrocarbon series, in which the number of atoms of each element forming a molecule increase by arithmetical progression, the lowest supposable combination will be the measure of the atomicity of the sum of the carbon atoms in every member of the series; for example, the body not yet isolated consisting of one atom of carbon and two atoms of hydrogen would have a combining power expressed by $4-2$; accordingly we find that ethylene, and every other homologue has the value of a dyad and may be united with two atoms more of hydrogen. Thus in the hydride of methyl series, the saturating power of a given number of condensed carbon atoms is expressed by the number of hydrogen atoms in the combination, which is the measure of the power of the same number of carbon atoms in other bodies; for example, the composition of hydride of ethyl proves that two atoms of carbon play the part of a hexad, therefore the saturating power of acetylene, composed of two atoms of carbon and two of hydrogen is expressed by 6—2. This branch of the subject cannot be presented even in outline without the use of symbols; and as the symbols I use are not yet generally adopted, and might divert attention from the principal points to be elucidated, I do not propose to introduce them in the present paper.

Our subject in its broadest meaning includes investigations relating to the diversity of composition in chemical bodies. The cause of this diversity may be assigned in a general way to affinity, by affirming that a powerful electro-positive element combines most directly and intimately with a powerful electro-negative, producing at once a compound in the most condensed state; as for example, sodium and chlorine in forming common salt, while elements having less affinity may unite by degrees in a variety of forms, as seen in the combinations of oxygen with nitrogen, sulphur and the halogens. This tendency to diversity is more remarkable among metalloids than among metals, for the latter, with one exception are solids, and in general, are not readily brought into the liquid and gaseous states.

Nothing in the whole range of nature's operations excites greater wonder than the fact that a large majority of the chemical bodies derived from the organic kingdom are composed of the same three or four elements, which by diversity of combination give as great a variety of characteristics. In ammonia, nitric acid, laughing gas, olefiant gas, ether, alcohol, aniline, water, spermaceti and the gaseous mixture forming the atmosphere, a combination of two or more of these elements exhibit the most striking contrasts. On examining hydrogen, oxygen, nitrogen and carbon, separately, among other peculiarities will be found the following:

1. Three of them are permanent gases which have resisted all attempts to liquify them by means of cold and pressure; and the fourth is a permanent solid, which in its purest form, the diamond, has maintained the same state under the highest degree of heat yet applied.

2. These elements are colorless under all attempts to change their natural state, yet by combination they evince every tint and shade of color.

3. Taking the weight of the hydrogen atom at 1, the combin-

ing numbers of oxygen, carbon and nitrogen are low multiples of the prime numbers 2, 3 and 7, respectively; from which the inference is drawn that harmonic relations exist between the vibrations or undulations produced by or in these elements. These coincidences may seem to have little significance, but when taken in connection with the remarkable relations of atomic weights of three elements in several different groups discovered by Dumas, and the general difference in the atomic weights of nearly all the elements, as pointed out by M. Carey Lea, they indicate the connecting links between form and motion, an allusion to which will be found in a previous paper on harmonic relations, and seem to confirm the surmise that all chemical elements are the offspring of the universal æther, first quickened by the divine Author of all, and ever moving in accordance with His stupendous plan.

To return to nitrogen; its connection with the highest prime number which belongs to harmony, would indicate that it does not frequently and readily enter into chemical combination. A natural inquiry arises here, whether any element supplies the missing prime number? The atomic weight of calcium is a multiple of the prime 5. This brilliant yellow metal enters into the composition of all bone, and, in combination with oxygen and phosphorus, forms the foundation of the animal structure. Ample reasons may be adduced for not finding in the atomic weight of phosphorus more simple numerical relations; however some significance must be given to the fact that phosphorus forms just one-fifth of the weight of animal phosphate. Two other elements entering into the animal organization are connected by similar numerical relations with those already mentioned, sulphur with oxygen, and iron with nitrogen. Other interesting coincidences might be pointed out did they not give too much prominence to views which at present will be regarded as merely speculative.

4. The normal saturating power of hydrogen, oxygen, nitrogen and carbon, are respectively as 1, 2, 3 and 4. Binary combinations of these elements will indicate the direction in which we are to look for the principal cause of diversity in the composition of organic bodies. Of hydrogen and oxygen only two combinations are known: of nitrogen and oxygen,

five; of hydrogen and nitrogen, one; of carbon and nitrogen, one; while of carbon and hydrogen over thirty have been thus far isolated. The remarkable tendency of the lightest gas and the most impenetrable solid to combine in definite proportions, and to condense by degrees, thus forming homologous gases of different densities, as well as liquids and solids of different boiling points, must be assigned as the principal cause of the diversity of composition found in the many hundred compounds of which these two elements form a part.

Berthelot has recently demonstrated, by numerous experiments, that alcohols, ethers, aldehydes, alkalies, acids and other compounds containing Carbon, may be decomposed by the action of hydriodic acid and heat, and converted generally into homologues of the hydride of methyl series; and he has expressed the opinion that the carbon in all organic bodies may be thus saturated with hydrogen. The startling conclusion to be drawn from his experiments, the facts set forth in this paper, and all previous experiments showing the disposition of hydro-carbons to combine with other elements is, that the great antipodes in extension, carbon and hydrogen, play the principal parts in the composition and decomposition of all bodies derived from the organic kingdom.

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20. On Some New Chemical Relations of Metallic Aluminum. By Professor Henry Wurtz, of New York.

The more important of the facts which led my investigations into this direction were observed in December, 1864; and some account of them was first offered at a meeting of the New York Lyceum of Natural History in November last.

To render myself fully intelligible at this time (especially as no publication has been made by the New York Lyceum, or otherwise, as yet) it is necessary that I resume concisely the whole subject from the beginning.

Aluminum, the most abundant of metals, has been deemed a chemical paradox. Though among metals one of the longest to elude isolation, and not known to exist in the native state, yet it is found to assimilate itself, when pure, with gold, platinum, etc., a class of metals always found native, and which varies diametrically from aluminum in their relations to electro-negative elements. My observations, as I believe, furnish the key to unlock this paradox.

In a paper in the "American Journal of Science," March, 1866, I have shown that aluminum presents a parallelism to most *native* gold in its relations to quicksilver; both showing normally a powerful superficial repulsion for the liquid metal, while in both cases the addition of a trace of sodium converts the repulsion into a positive and powerful attraction.

The analogy holds good also in another point, namely, that mere abrasion or incision of either metal, thus developing an artificial surface, shows immediate adhesion of the quicksilver to such new surface. Here, however, the analogy ceases, and in fact this adhesion is far more difficult to exhibit with Al than with Au, the former metal presenting the singular phenomenon of an extension of the lamina, which is passive or repellent to quicksilver, to some appreciable depth under the surface; while in the case of Au this lamina is but the thinnest possible film. Hence it is, probably, that the remarkable relations of Al to Hg escaped observation until my experiments with amalgam of sodium. Also as I have remarked in the paper before cited, while Au is quickly penetrated by Hg at the points of contact, forming an amalgam; Hg after being made to adhere to Al, either by rubbing, or by Na, is readily "wiped off again clean, as water may be from glass." In this latter point the analogy of Al is stronger with platinum than with gold.

Both cast and wrought aluminum behave alike in these respects. If a piece of sheet aluminum, with edges freshly cut or broken, be instantly immersed under quicksilver, penetration of the latter at such edges generally commences at once, and proceeds rapidly; so that in a brief time the sheet *may be split* (either partially or wholly) into two, brightly enfilmed on their inner surfaces. The phenomenon is still more strikingly shown by bending sharply under quicksilver a piece of tolerably thick sheet aluminum. Sometimes at the first bend the metal, otherwise so tough, breaks short off, generally with a strongly marked laminated fracture, the two exterior layers parting along separate lines, so that brilliantly enfilmed internal surfaces are exposed.

If such enfilmed surfaces be now exposed to the air, best after wiping off the excess of quicksilver adherent, a singular phenomenon will be witnessed. Their brightness is instantly tarnished, and in a few seconds *an efflorescent growth appears*, so rapid as to be visible to the eye; this efflorescence being composed, as I have found, of pure hydrate of alumina, in the form of single threads and filamentary bunches. Under the magnifier the growth of the feathery mass of slender curved filaments on such a surface of aluminum, is a sight of a very peculiar and unprecedented character. The more moisture there is in the air the more rapid the action. The evolution of heat accompanying the combustion is readily perceptible to the hand; and, by certain modifications (sodium being used to effect a rapid enfilming) may easily be made intolerable thereto!

Two new and startling conclusions are thus forced upon us, which I shall put into the form of axioms or postulates of a new theory of the nature of aluminum, to wit:

1. Our ordinary or normal and stable form of aluminum is in reality correspondent to what is known as the "passive" (usually highly *unstable*) form of other metals (as the well-known "passive iron" for example); or, as I should prefer to say:—the resultant of the forces that aluminum encounters when isolated, under ordinary conditions, places its surface in an *electronegative* state or *attitude* towards oxydizers.

2. Absolute *molecular* or *chemical* contact with quicksilver produces an extraordinary, unstable and abnormal form, corresponding to the normal forms of sodium and potassium; or, as I would express it:—quicksilver, by absolute contact, *induces* upon the surface of aluminum that intensely active *electropositive attitude* towards oxygen and water which we have hitherto known only as an attribute of the alkali-metals.

In my experimental investigation of this theory, the following questions and hypotheses have been examined:

Question 1. Most essential of all. May it not be due merely to a very fine division or diffusion, as in an amalgam of Al?

A negative reply is inferable from the fact that fine division otherwise produces no such phenomenon; but a crucial test is to attempt to produce, what on this hypothesis should be producible, a solution or fluid magma of Al in Hg presenting the phenomenon. Neither with hot nor cold Hg, with nor without Na, can any such solution or magma be obtained. In fact, even if the coating of Hg on the Al has an appreciable thickness, no action takes place, except at the edges of immersion.

The penetration I have described of Hg between the laminæ of a sheet of Al appears similar to that in Professor Henry's lead-syphon.

Question 2. Is the new phenomenon to be classed with those of the *pyrophori* (like iron reduced by H, Pb from the ignited tartrate, etc.) which burn spontaneously to anhydrous oxyds, independently of atmospheric moisture? Answered readily by enclosing enfilmed aluminum in jars of air previously dried by sulphuric acid and by chloride of calcium. It was found to remain bright for hours.

Question 3. Is the phenomenon then analogous to those of K and Na, when exposed to the air; and accompanied by the production of a hydrate? Ignition of the effloresced product in a glass tube, and collection of the condensed water, answered this question in the affirmative.

I infer from these facts that unless we have here come upon an entirely new class of relations, destitute of any known analogue, we must conclude that Al in this new state corresponds with the ordinary states of the alkali-metals. The possibility of passive forms of K and Na is here suggested; and that of a permanently passive state of iron, corresponding to the ordinary form of Al. This latter possibility is not without support on other grounds; as analogies with other metals, and particularly the many recorded cases of accidental production of iron highly resistant to oxygen; insomuch that most chemists find it necessary to explain its usually rapid rusting, by suppositions of *voltaic* actions arising from lack of homogeneity, and so on; thus implying a belief in a natural highly resistant, if not wholly passive form. I may add that the passive state if discoverable, will doubtless be found inherent only in a *pure* iron, hardly attainable by the modes of manufacture in use; and suggest trials by modes analogous to that for Al, namely by decomposition by Na of ferrous or ferric chloride or other haloid iron compound in anhydrous form. Pertinent hereto is the consideration of the familiar modes of producing external passive films on commercial iron, involving voltaic conditions not at all understood.

Much time has been spent by me in testing the question of the existence of an amalgam of Al or of any real solvent action of Hg upon Al, but with negative results. Fine filings or scrapings of Al or *leaf* Al may be incorporated with Hg, by rubbing in a mortar, into a pasty mass. This mass, however, does not exhibit the phenomenon of spontaneous combustion, simply because of the *complete* envelopment of each particle of Al by the Hg; all access of atmospheric moisture to the former being thus debarred. An experiment, remarkably confirmatory of the opposite internal and external conditions of Al, and in other respects highly curious, is made by simply throwing a piece of Al leaf upon a clean pool of Hg. Instant and powerful adhesion is manifested, the leaf flattening itself out just as a leaf of Ag would under the circumstances. This appears, however, to be due merely to the *cohesive* attraction of the two films of air condensed upon the two metallic surfaces; for no spontaneous oxydation of the Al ensues, after ever so prolonged contact. If now the edges of the leaf be simply *torn*, while lying on the Hg, at every such torn edge real *chemical adhesion* is developed, and a conversion of the leaf into a white powder of hydrate of alumina will commence, which will propagate itself, though *with extreme slowness* across the whole. The slowness of the change in this case being clearly due to the fact that the action of the Hg is but through the excessively tenuous internal lamina of the leaf, which is not passive to Hg.

The inference might be made by some, that it will be necessary, to avoid presumed danger from spontaneous combustion, to keep Al and Hg carefully apart, at least when in any considerable masses; but, for several reasons (though such precaution will actually be necessary on other grounds) little danger may be apprehended on the ground of *fire:* 1st, because of the very high capacity of Al for heat; 2d, because of the rapid development of an obstacle to the access of moist air, through the accumulation of the feathery hydrate; 3d, and most conclusive: through the quick dissipation of the exciting agent, the Hg, by elevation of heat. Enfilmed Al, when heated above ebullition of Hg, assumes a surface which, while dull and metallic, is again passive.

Still, contact of Hg will be destructive, to a ruinous extent, to all articles made of Al; a conclusion from my observations of obvious practical and commercial importance.

Enfilmed Al in water produces but a slow effervescence, and the question occurs whether the insolubility of the hydrate interferes. Addition of sulphuric acid, however, does not set up any more rapid action; so that it seems likely that the intensity of the action in air may be due to coöperation of the free oxygen of the air.

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21. On the Condition of our Knowledge of the Processes in Luminous Hydrocarbon Flames. By Professor Eugene W. Hilgard, of Oxford, Mississippi.

For more than half a century after Sir Humphrey Davy's important investigations of the subject of flames, the experiments and explanations of that eminent philosopher have passed, unchallenged and almost unchanged, into text-books and lectures. As regards luminous flames especially, he established the necessity of the presence of a solid incandescent body to produce useful luminosity; and in reference to the flames of hydrocarbons in particular, he suggested that the liberation of carbon in them was owing to the combustion of the hydrogen of the compound in advance of its carbon, the latter being heated to incandescence by an oxyhydrogen flame, as it were, and failing to be consumed until all the hydrogen was first oxidized.

It is remarkable that an explanation so directly at variance with the daily experience of blacksmiths, and with a lecture experiment performed even in the most elementary course of chemistry, could so long have passed current; for the decomposition of steam by ignited charcoal into hydrogen and carbonic oxide gases is an old observation. Nay, most of us who have performed this experiment on the lecture table, have been in the habit of passing the gas through lime water or potash lye, to free it from *carbonic acid* before showing its properties. And yet we were taught, and have ourselves taught, that in the flame hydrogen burnt first and carbon afterward, and any doubt on the subject was quieted by a reflection about the influence of "chemical mass" on affinity.

When, in 1852, I was a student in the laboratory at Heidelberg, to which Bunsen had then but just been called, he suggested to me as an interesting subject of investigation, the composition of the gases in the various portions of the flame; stating at the same time that the prevalent opinion concerning the respective affinities of hydrogen and carbon for oxygen were certainly erroneous: that when mixtures of carbonic oxide and hydrogen were exploded with oxygen in the eudiometer, the distribution of the latter took place according to some law dependent upon the relative proportions between the combustible gases, and also the amount of oxygen present, but discriminating widely in favor of carbon. The experiments upon which this opinion was based are, probably, those given in his "Gasometric Methods" at the conclusion of the chapter on the combustion of gases.

In order to study the processes taking place in the flame, it became necessary to investigate first of all the composition of the gases in the interior cone, from which the flame derives its substance. I constructed a lamp adapted to the introduction of a suction tube into the flame from below, and made a series of fifteen analyses of the gases so collected at various points of the interior cone of the flames of beef tallow, and of wax. The low temperature known to prevail in this portion of the flame rendered it least adapted to elucidating the mooted question of affinities; but being the generator from which the other portions of the flame are supplied, a knowledge of its component gases was indispensable. My analyses (published in Liebig's Annalen der Chemie und Pharmacie, vol. xcii, p. 129, 1854) showed, however, the existence even in the highest portion of the cone, of free hydrogen with a large excess of carbonic oxide and carbonic acid; the amount of hydrogen varying but little from base to point, while the carbonic acid increased in about the same ratio as nitrogen, *i.e.*, in proportion to the oxygen entering the flame. Bunsen as well as myself failed, however, to draw the legitimate conclusions from these facts at the time; the more as, with the materials I used, it was impossible to follow the formation of water by progressive oxidation.

The latter difficulty was avoided by Landolt, who, two years later, took up the same subject, the failure of my health having rendered doubtful the prospect of my ever being able to resume it, so as to carry out the proposed investigation of the other parts of the flame.

Landolt* used illuminating gas of known composition, and was therefore enabled to determine the deficient factor in my analyses, viz., water. So far as comparable his results in general confirmed mine. He felt compelled by the increase of free hydrogen in the higher parts of the flame examined by him, to assume the occurrence of a reaction between free carbon and (pre-formed) water; but he also failed to draw the inevitable conclusion as to what *must* happen in the luminous cone.

A later research of Lunge† on the composition of the gas contained in the interior cone of the flame of a Bunsen's burner, must have led to the truth of the matter, by showing how little oxygen sufficed to render a flame non-luminous when previously mingled with the gas. I have not seen Lunge's memoir; but he likewise seems to have failed to draw the important conclusion of which his analyses must have contained the elements.

Next, in June, 1860, comes a memoir of Erdmann,‡ who in discussing the principles upon which his gas-tester (a modification of Bunsen's burner) is based, first distinctly enunciates that, according to his experiments, the carbon in a flame is oxidized *before* the hydrogen, and that the separation of carbon upon which luminosity depends, is due to heat alone, as would be the case were the gas passed through a red hot tube.

Finally, eighteen months later, we have an elaborate and

* Pogg. Ann., vol. xcix, p. 389.

† Annalen der Chemie und Pharmacie, vol. cxii, p. 205.

‡ J. pr. Chem., June 1860, p. 241.

elegant research by Kersten,* wherein he proves by eudiometric experiments that (at least within the limits of the proportions employed by him) whenever a hydrocarbon is exploded with oxygen insufficient to burn more than the carbon to carbonic oxide no hydrogen at all is oxidized; but that as between carbonic oxide and hydrogen, the formation of carbonic acid on one hand and of water on the other depend upon "chemical mass," as Bunsen had already shown.

This question has therefore been peremptorily settled by decisive experiments, as much as eight years since. Yet the latest editions of text-books published in this country, and even such as, like the excellent work of Messrs. Eliot and Storer, claim to represent the latest state of the science, still retail the old error regarding the succession of oxidation.

There is another point which, though I took special pains to demonstrate the facts fourteen years ago, is still incorrectly stated in almost all text-books as well as books of reference. I allude to the definition of the several essentially distinct parts of the flame. *Three* only are usually mentioned, viz., the inner cone, the luminous portion, and the outer, faintly luminous envelop or veil. Yet Berzelius already distinguished the very important fourth part, viz., the blue cup-shaped zone surrounding the base of the flame, which is as sharply defined from the inner cone, as from the outer veil with which it is usually confounded.

That this blue cup is identical with the blue cone of the blowpipe oxidation-flame, is stated by Plattner in the first edition of his work on the blowpipe. Strangely inconsistent with his own definition, he nevertheless teaches that the blue cup is formed by the combustion chiefly of carbonic oxide, produced by the "first and weakest effect of the heat on the fuel"— an assumption as little justified by experiments regarding such action, as was that of the combustion of hydrogen previous to carbon. It is palpably inadmissible in reference to the blue oxidation-cone, which is of course identical with the flame of a Bunsen's gas-burner—the supply of oxygen being, in both cases, sufficiently great, and so mingled with the entire gas, as to suppress the separation of carbon.

* J. pr. Chem., Dec. 1861, p. 290.

The part performed by the blue cup, viz., that of a self-heating retort with walls impervious to oxygen, in which dry distillation is accomplished; its theoretical import, as the counterpart of the luminous portion, where the same gases are burnt with evolution of light, render the neglect with which it has been treated doubly surprising. That it is totally distinct from the outer veil is readily perceived when the eye is protected from being dazzled by means of a screen of the shape and size of the luminous hollow cone. The veil is then seen surrounding the blue cup as well as the higher portions of the flame, and is thus proved to be nothing more than a zone of glowing gas; which of course, however, cannot be strictly defined from the luminous envelop, the oxidation being a gradually progressive one, from the highly luminous central portion to that brownish, semi-transparent zone of transition, where the carbonic oxide, burning simultaneously with hydrogen, fails to produce its characteristic blue tint because of the excessive temperature existing there. The same is the case when it is burnt by itself from a jet kept at a white heat.*

The inner cone, too, is still incorrectly defined as "the space containing the combustible vapors and gases generated from the wick." This would lead any one to suppose, that the external atmosphere had only the effect of burning off the outside of this gaseous mass, and some text-books have gone far in the graphic delineation of this process. My analyses first proved, fourteen years since, that even in a tallow flame, characterized by an excess of fuel over the available oxygen, the per centage of nitrogen gas does not fall below fifty-nine per cent. in the lowest part of the flame, and increases to seventy-six per cent. at the point of the inner cone, with from ten to fifteen per cent. of carbonic acid. The products and educts of combustion therefore greatly exceed the combustible gases, and essentially modify the processes thereafter occurring; for it is clear that in the luminous portion, the water and carbonic acid must in part at least again yield up their oxygen, before final combustion.

On the latter point, I have never been able to agree with Bunsen, who maintained that the combustion of the carbon in

* See Gmelin's Handbook, art. Carbonic Oxide.

the luminous envelop must be effected by free oxygen penetrating into it; because its combustion at the expense of the reduction of carbonic acid and water, could not be the cause of such intense production of light and heat; and upon his authority I reluctantly adhered to this view in my paper, above referred to. I believe with Kersten (l. c. p. 314), that the burning surface of the luminous cone, where carbonic oxide and free hydrogen meet the oxygen of the air, is the sole source of the heat which causes the intense ignition of the interior, and that the oxidation of the free carbon is effected only through the medium of the intensely heated carbonic acid and steam which penetrate from the outside. I habitually compare the process taking place in the luminous envelop, to what would happen if illuminating gas were passed through an ignited tube into which steam and carbonic acid are injected through lines of lateral orifices. The ignited carbon set free by heat would render the interior atmosphere intensely luminous at the near end, but the luminosity would rapidly decrease toward the far end, where we would have such a mixture of carbonic oxide and hydrogen as that which, I conceive, is burning on the surface of the luminous cone, and which *there*, above the point of the latter, produces the maximum temperature.

Unfortunately, a direct solution of the question by an investigation of the gases contained in the luminous envelop, is exceedingly difficult. Much remains to be done in the study of the details of the differentially variable processes by which the familiar phenomenon of a luminous flame is produced; but what *is* known is at least worthy of a place in the text-books.

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22. The Meteorites from Poland and Mexico. By Dr. Lewis Feuchtwanger, of New York.

(ABSTRACT.)

The specimen here exhibited fell 30th January, 1868, in Pultuosk, near Warsaw in Poland, and I saw a short notice of it in a public print, of the wonderful Bolis of Warsaw, stating that on a starlight night the citizens of Warsaw gazed petrified with fear at the rapid approach of an immense ball of fire, which at last bursted over their heads with a noise and shock, such as never had been heard or felt before on the surface of the earth. After the globe bursted, each of the pieces in turn broke up, until parts of the mass, before reaching the earth, were broken up in small masses accompanied by discharges resembling those of artillery, and several of them resembling more nearly those of many regiments of small arms; and during these several discharges millions of pieces, mostly rounded masses with a black metallic coating, were found scattered in the neighborhood, which from appearance are the Lithosiderites according to Shepard's last classification. Mr. Daubree has presented to the French Academy 942 pieces of these broken bodies, and an analysis of their exact composition may be expected from that gentleman, but I have not seen any notice of it as yet.

The Mexican Meteorite. But a small piece could be chiselled off on account of its extreme toughness from a lump of 800 pounds weight; in all were eight pieces of the meteoric irons landed in this city August 1st, weighing in the aggregate about 4 000 pounds, from a bark sailing from Texas from where they were sent by Dr. H. B. Butcher, who procured these masses in the spring of 1868, but they were known to exist ever since 1837 by Dr. Long, who resided then in Santa Rosa, and who gave a description of the phenomena attending their appearance, of the excitement produced by many citizens who were also eye witnesses of that catastrophe; and that gentleman stated that he went in company with many friends in search of the locality where these meteorites descended, about ninety-five miles west of Santa Rosa, but was then unable to trace the spot.

Having but a small piece at command for analysis, I took repeatedly the specific gravity and found it to be 7.50 and a hardness of 3; its color is silver-white, the form of the masses is quite angular, but rounded at the ends, a number of air holes from one to two inches in extent is visible on all the eight masses, and from first appearance they resemble animal remains.

The chemical analysis yielded me ninety per cent. iron, nine per cent. nickel, and about one per cent. composed of other foreign substances, with a trace of manganese and cobalt; by treating the small piece at my disposal with nitric acid, the Widmanstidi figures were very distinct. It was impossible for me to ascertain whether the Schreibersite, Troilite or Chamasite can be traced.

Having reflected lately on the subject, and comparing the remarks of Mr. Forrest Shepard in his letter to Professor Charles U. Shepard, of article forty-seven, page 347, in "American Journal of Science and Arts," 1866, of Major Hamilton's journey from Santa Rosa westerly to Naricmento about ninety miles north-west from Santa Rosa, who described the narrow valley of a quarter of a mile square partially overgrown by palmetto palms, and there he saw fourteen ponderous masses of native iron, the largest of which rose upwards of four feet above the ground, having the form of a bee-hive. These masses Professor Shepard designated as the Boranza Meteorite, which, according to his reported chemical composition, appears to correspond with the results obtained by me. Mr. S. states that the residue form the solution in nitric acid and was of a pulverulent nature, which was also the case in my examination, and I am forcibly impressed with the idea that the fourteen masses spoken of by Mr. Forrest Shepard, are identical with the eight masses which Dr. Butcher procured in Mexico.

It is likewise apparent to me that Major Emory's iron masses having been found ninety miles north-west from Santa Rosa, 28° latitude and 101° longitude, are identical with these eight masses. I am farthermore inclined to the belief that the meteorite in the Smithsonian Museum at Washington, weighing 250 pounds, and stated as having occurred but fifty-six to sixty miles from Santa Rosa, Coahuila, is one of the fourteen masses of iron spoken of by Major Hamilton.

There is no doubt but what the fall of those iron masses created great excitement in northern Mexico in the department of Coahuila about thirty years ago; the eight masses now in Philadelphia are unquestionably a part of the fourteen masses mentioned. The inhabitants of that neighborhood were all cognizant of the existence of all these masses. They all fell at one time and at one and the same locality.

23. THE STATICS OF THE FOUR TYPES OF MODERN CHEMISTRY, WITH ESPECIAL REGARD TO THE WATER TYPE. By Professor Gustavus Hinrichs, of Iowa State University.

IT has taken exactly half a century to discover and fully develop the fundamental law of chemical combinitions; and this law is a paradox, for it states that

$$1+1=2 \qquad 1+3=2$$
$$1+2=2 \qquad 1+4=2$$

and in general $m+n=2$, for the component and resultant volumes of elements and compounds in the gaseous condition. The memorable experiment of *Gay-Lussac* in 1805, establishing the combining ratio of oxygen and hydrogen to form water as one to two, opened this line of research; in A. W. Hoffman's admirable "Introduction to Modern Chemistry, 1865," the law is clearly stated, as above, and fully supported in all its bearings by ingenious and decisive experiments.

But what is the theoretical consequence of this remarkable law, and what is its cause? This question seems not yet to have been touched upon, except in my *Atomechanics*, June, 1867. We shall here consider this question independently of the constitution of the elements.

In older branches of physical science similar laws are known and explained. Thus one, two, or three or four equal weights may balance the same power, by means of a lever or a hydraulic press; but in this case the law has long ago been fully explained and therefore ceased to be a paradox.

In chemistry, the law is in general as follows: The volumes of two elements A and B in gaseous condition combine according to some of the following modes or *types:*

1 volume	A + 1 volume	B	giving	2 volumes		AB.
1 "	A + 2 volumes	B	"	2	"	AB_2
1 "	A + 3 "	B	=	2 volumes		AB_3
1 "	A + 4 "	B	=	2	"	AB_4

But besides these types there are many others, as

2 volumes A + 3 volumes B = 2 volumes A_2B_3

and indeed, organic compounds prove that in general

m volumes A + n volumes B give 2 volumes A_mB_n

The high theoretical interest of this law is very much increased yet by the fact that the elements have a definite predilection for some one of these types (*atomicity* of the elements). This is represented, together with my very convenient general symbols (*greek* initials) of the groups of elements, in the following table:

HINRICHS' CLASSIFICATION OF THE ELEMENTS.

Metalloids:		*Metals:*
	MONATOMIC.	
Chloroids, Χ; Fl, Cl, Br, Io. *Pantoids*, Υ; H.		*Kaloids*, Κα; Li, Na, Ka, Rb.
	DIATOMIC:	
Sulphoids, Θ; O, S, Se, Te.		*Chalcoids*, Χα; Ca, Sr, Ba. *Cadmoids*, Κδ; Mg, Zn, Cd, Pb.
	TRIATOMIC.	
Phosphoids, Φ; N, P, As, Sb, Bi.		*Ferroids*, Σι; Al, *Fe*, Rh, Ir. *Sideroids*, Σδ: Cr, Mn, Fe, Ni, Co.
	TETRATOMIC.	
		Titanoids, Ττ; C, Si, Ti, Pd, Pt,

and several other groups. These are sufficient for our present purpose (*Atomechanik*, Abschnitt I). The symbols here introduced for natural groups of elements will be found exceeding convenient.

The law of combining volumes is entirely independent of the nature of the combining elements, and must accordingly be due to a simple *mechanical* cause. Thus both H_2S and O_2S are following the same law $2 + 1 = 2$ volumes, although in H_2S we have two atoms of a monatomic element, but in O_2S two atoms of a diatomic element. So also N_2O and O_2N follow precisely the same law; and yet in the one we have two atoms of nitrogen, but in the other two atoms of oxygen.

Hence it remains to find the mechanical cause of this condensation; but that can only be done by referring to the constitution of *gaseous* elements as now understood. It matters nothing, whether we take the molecular or the atomic view, that is whether we suppose the formula of Hydrogen to be H (1 vol.) or H H (2 vols.). We shall adopt the former view, because the latter appears to be disproved by *Tyndall's* experiments on the absorption of radiant heat. (See my paper in the *American Journal of Mining* for March 2, 1868.)

Let us then represent a number of atoms of hydrogen by an equal number of adjacent and equal squares, as *Hofmann* does; thus the same number of atoms of any other truly gaseous element will be represented by the very same figure, so far as magnitude of the *space occupied* is concerned.

But it is very often forgotten that the ponderable atoms far from FILL these atomic volumes (squares representing *cubes*), and that the *degree of condensation* for the different uncombined elements is very different.

In regard to the first circumstance, it is well known from the excessive increase in volume upon the conversion of a liquid into gas, and by the high expansibility and compressibility of gases themselves, that the solid, ponderable particle—called the ATOBAR in Atomechanics—FILLS but a very small portion of the *atomic volume* occupied by this atom, that is inside of which there is no other atobar. Whether it be vibratory caloric motion (*Krönig*, *Clausius*, *Tyndall* and others) or whether the particles (atobars) be surrounded by an elastic, repellent medium (ether; see older and also present writers, *Tyndall*) this question need not concern us here. But it may be well to give one or two illustrations. A minute *dot* in the centre of the square representing the atomic volume will therefore indicate the active *atobar*. So also we say a person of about 5 cubic feet occupies a room, say 15 by 10, by 10 or 1500 cubic feet; hence only about $\frac{1}{300}$ of the actual space is *filled* by the person! The same room will hold a visiting friend or two, and be not actually *filled* yet. So inside the space of our earth's orbit only $\frac{1}{10000000}$ part of the globular space is filled by the sun, and yet the sun *occupies* and pervades by its light, heat and gravitating power, a much greater space, reaching beyond the stars!* So it also is in matter, at least in the gaseous condition. The various elementary atoms combined are as the physical double and multiple stars in the heavens.

In regard to the second point it may be enough to call to mind that one atomic volume of hydrogen weighs *one*, but the same atomic volume of iodine weighs 127, hence is 127 times as much filled as the hydrogen atomic volume. So also one

*Of the globe described by a radius equal to the distance to 61 *Cygni*, the solar system only *fills* $\frac{1}{2.10^{24}}$ part (2 followed by 24 zeros!).

atomic volume of oxygen contains 16 times as much of matter, as one atomic volume of hydrogen. From this we can readily see how one, two, three or more hydrogen atoms (their ATOBARS) may be brought into the same atomic volume occupied by but *one* atom of uncombined hydrogen.

Here it is important to notice the following direct consequence of condensation. If *two* atoms of hydrogen are condensed to one atomic volume, their two atobars form a straight line (two points always determine a straight line). If *three* hydrogen atoms are thus condensed, not only do these three atobars determine a plane and the three corners of a triangle, but they will form an *equilateral triangle* (of 60 degrees) because the three atobars being alike, their mutual action will be alike upon one another. So also if *four* are united to one group inside of *one* atomic volume, then four will form a *square*, unless some other force deviate them more or less from this position, just as for example the sun does deviate the moon from her elliptical orbit around the earth.*

These simple developments will enable us to understand the fundamental law of volume. We shall consider the four principal types one after the other.

TYPE AB. This is a simple juxtaposition with probably only a slight approaching of the two atobars A and B towards one another. Hence vol. A + B = vol. AB or for these atom-volumes: $1 + 1 = 2$. After combining A and B *move as one body*, if we suppose that the atobar occupies the atomic volume in virtue of its vibratory heat-motion; or, if we accept the view of ether-envelopes, we must suppose the two atobars in perfect contact with their ether-atmospheres. The best known example is HCl; also the Kaloid-Chlorids, *Ka* Cl, or more generally the Kaloid-Chloroids, *Ka X*. The practical importance of my symbols is best understood by remembering that this formula *Ka X* embraces the following sixteen compounds, viz.: Li Fl, Li Cl, Li Br, Li Io; Na Fl, Na Cl, Na Br, Na Io; Ka Fl, Ka Cl, Ka Br, Ka Io; Rb Fl, Rb Cl, Rb Br, Rb Io.

Type AB_2, or *Water Type*. Considering the three atobars of

*It need hardly to be accentuated that the condensation here spoken of is very different from that produced by mechanical compression. In this the atobars move separately, but when brought to *one* atom they move as one body.

A, B and B, they form a plain *triangle;* and if we as a *first* approximation consider them *equal* material points, this triangle must be EQUILATERAL as the only stable figure of equilibrium $A\{^{B}_{B}$. But at all events AB_2 is *isoceles*, because each B is like the other, so that BB forms the base of this triangle. According to the difference between A and B, the triangle will deviate from being equilateral.

Let the axis from the centre of gravity of the triangle BAB to A be called z, and the other at right angles hereto in the plane AB_2 y, then we evidently have

1° for A and B considered as equal points

$$y:z=1:\sqrt{3} \qquad \sqrt{3}=\text{tg } 60:$$

2° for A not being exactly equal to B, a small deviation must take place; small because the unlike atobars of A and B are always at relatively great distances from one another; hence we may put

$$y:z=1:\sqrt{3}+\zeta$$

where ζ is a small, positive or negative quantity (which we will term the *deviation* or perturbation).

Now the two equal atoms B have always their centre of gravity midways between them; hence AB_2 is statically the same as atobar A and atobar 2B at the centre of gravity of B and B. In other words it is of the first type only that B_2 is represented by the centre of gravity of these B_2 or by 2B. And since now a great number of atoms B may be brought within *one* atom volume, these two B fill only one when combined, so that with the original atomic volume of A the atomic volume of AB_2 is *one volume* of A plus *one* volume B_2 giving in all *two* volumes of AB_2.

The isoceles triangle is, as it were, indicated by the typical formula $A\{^{B}_{B}$. But that this never was intended for such a purpose is well known and abundantly substantiated by the customary typical formulæ $A\left\{\begin{matrix}B\\B\\B\end{matrix}\right.$ and even $A\left\{\begin{matrix}B\\B\\B\\B\end{matrix}\right.$ in our chemical works.*

*It has often been assumed that AB_2 forms a straight line (*Gaudin*). But although AB_2 *can* form such a line, it does not retain it, heat and other vibrations would bring it out of this position, and immediately carry it farther.

TYPE AB_3. By reasoning as above we learn that this type is produced by the three B forming a triangle (equilateral).

Hence 3 vol. B are condensed to one volume, which, uniting with the one volume of A gives two volumes of the compound AB_3; the three atobars of B forming a triangle (equilateral or nearly so) above the atobar A.

TYPE AB_4 results in the same way. The four volumes of B condense to *one* by the uniting of the four atobars to one system; this one volume uniting with the one volume of A produces the two volumes of the compound. The form of the four atobars B is that of a square or nearly so; the centre of gravity of this square or of 4B constitutes as it were the new (compound) atobar which unites with A just as in the first type, without condensation, by simple juxtaposition.

In precisely the same way all other binary compounds are formed; thus in A_2B_3, the two volumes of A unite to one volume, and the three volumes of B also condense to one volume: these by juxtaposition produce two volumes of A_2B_3, wherein the three B form a triangle (equilateral or nearly so) and the two A, a straight line.

If now the compounds are to be represented by means of their symbols, we must write

$$\overset{B}{A} \qquad \overset{B\,B}{A} \qquad {}^{B}\overset{B}{A}{}^{B} \qquad {}^{B}\overset{B}{A}{}^{B}\;{}^{B}$$

or of the atobar B, be indicated by a star only

$$\overset{*}{A} \qquad \overset{*\,*}{A} \qquad *\overset{*}{A}* \qquad *\overset{*}{A}*\,{}^{*}$$

and if also the atobar A, be indicated merely by a cross,

$\overset{*}{x}$	$\overset{*\;*}{x}$	$\overset{*\;*\;*}{x}$	$\overset{*\;*\;*\;*}{x}$	In perspective.
x	$*\,x\,*$	$\underset{*\;\;*}{\overset{*}{x}}$	$\underset{*\;\;*}{\overset{*\;\;*}{x}}$	horizontal projection.

It will be seen, that two of the types are written by chemists in accordance with the real structure of the compound atom. The first, because two points A and B can only form a straight line; hence we have AB, or specially HCl, etc. So also the water type is written $\left.\begin{smallmatrix}B\\B\end{smallmatrix}\right\}A$; for instance water $\left.\begin{smallmatrix}H\\H\end{smallmatrix}\right\}O$, thus giving an exact representation of the isoceles (or equilateral) triangle formed by the three atobars.

But that no such representation was intended, is evident enough, and may also be seen from the customary method of writing the other types:

seen from above:

$\left.\begin{matrix}H\\H\\H\end{matrix}\right\}$ N instead of $_{H}{}^{H}{}_{H}N$ 

and $\left.\begin{matrix}H\\H\\H\\H\end{matrix}\right\}$ C instead of $_{H}{}^{H}{}_{H}{}^{H}C$ 

The constitution of the four types here given has been shown to be a mechanical necessity; hence experience must confirm it:

I. By the constitution and all *reactions* of the various compounds;

II. By the *physical properties* peculiar to the different compounds; and

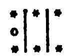
III. By the *crystalline form* of the compounds.

In regard to the first, the *constitution* and *reactions* of bodies, we can only give one example here; *Alcohol*, C_2H_6O. Its constitution resulting from the above types (AB_4 and AB_2) is represented

Longitudinal view:

1. Vertical; $h = H_2$ 1, $\overset{O}{{}^{h}C^{h}}\,C^{h}$

2. Vertical, C = |, H = • 2,  4, 
Front view.

3. Horizontal projection: 3, 

The first formula is especially convenient; *h* is printed for H_2 because both atobars H are seen at the same spot. The horizontal projection shows some reactions plainer than the vertical projection.

In regard to *structure*, we see that each C is acted upon by four H, each O by two H; in accordance with the atomicities of O and C. The atom possesses a *stable* equilibrium. Farthermore we see that the various views in regard to its constitution are all embraced in this true form; for all were based upon *some* experimental evidence.

Thus figure 1 shows that alcohol may be considered a monoxide of $C_2H_6 = Ae$, or alcohol $= Ae\,O$.

Again, figure 3 or 4 shows that it is built according to the water type, there being in the place of one H the body HCH_2

CH_2 or empirically $CH_5 =$ ethyl-radical; hence the type formula:

$$\left.\begin{matrix} C_2H_5 \\ H \end{matrix}\right\} O.$$

The modifications of alcohol are also fully represented; as we shall show by a few of the most characteristic reactions.

Oxygen can act upon the terminal H_2 (fig. 1, 2, 3) which are removed, leaving

Aldehyde C_2H_4O $\quad \overset{o}{{}_h}C\,{}^{h}C$

and this vacant space will by exposure to oxygen be occupied with it, forming

Acetic acid $C_2H_4O_2$ $\quad \overset{o}{{}_h}C\,{}^{h}\overset{o}{C}$

By means of proper hygroscopic bodies the water atom may be removed intact from the one extremity, giving ethen (olefiant gas) and water

$\overset{o}{{}_h}C^{h}\,C^{h}$	gives	$\overset{o}{h}$	and	$C^{h}\,C^{h}$
alcohol		water		ethen

Again, by passing the vapor of alcohol through a red hot tube, it will be decomposed, so as principally to *shake off* the terminal water H_2O and H_2, as shown by

$\overset{o}{{}_h}C^{h}\,C^{h}$	gives	$\overset{o}{h}$	and	$C^{h}\,C$	and	h
alcohol		water		acetylene		hydrogen
C_2H_6O		H_2O		C_2H_2		H_2

These few examples must be enough here; they indicate already several reactions which cannot be accounted for either by the older views, or by the type theory. The late attempts of *Frankland*, *Erlenmeyer* and others, show that such theory is urgently called for by the demands of the rapidly advancing science.

Concerning the *physical properties* time forbids to go into any details at this place; I therefore beg leave to refer to the third section of *Atomechanik*, to another paper to be presented to this Association, and to my paper on *Tyndall's* measurements of the absorption of radiant heat by gases, which values have been calculated by me on the basis of the constitution of matter here presented. (Am. Journal of Mining, May 2, 1868).

Lastly, *the* CRYSTALLINE FORM *of the compound AB_2 confirms the structure here given.* From the centre of gravity of the equilateral *tritoid* AB_2 draw an axis z through A; the axis at right angles thereto is y, and since AB_2 is an equilateral triangle, we have

$$z = y \text{ tg } 60° = y\sqrt{3},$$

or

$$y : z = 1 : \sqrt{3} \qquad (1)$$

This is *the normal form*, resulting if A and B are equal, or nearly enough equal to one another that the difference between them is not perceptible at their great relative distances. But if A have a very great affinity for B, if the weight (mass) of A be very much in excess of B, then z is somewhat smaller than

$$\sqrt{3} = 1.73205 \qquad (2)$$

and will be represented by $\sqrt{3} - \zeta$, where ζ is the *deviation* or *perturbation* of z due to the deviation of A from equality with B.

By aggregation the crystallographic axes are necessarily in the same ratio as these atomic axes, so that y and z also stand for the *crystallographic axes;* for the CRYSTAL IS SIMPLY AN AGGREGATE OF ATOMS IN PARALLEL POSITIONS, so that the labors of *Bravais*, *Frankenheim* and others become directly applicable.

From the ratio here given it follows that the prism parallel to the axis x or at right angles to the plane yz must have the angles of 120° and 60°, if $\zeta = o$, or nearly this value if ζ not equal to zero. In other words the *tritoid AB_2 is truly hexagonal or nearly so.*

The vertical axis x is determined by simple aggregation. The *cube* being the simplest case, will give

$$x = \sqrt{\tfrac{3}{2}} = 1.22474,$$

or the form

$$x : y : z = \sqrt{\tfrac{3}{2}} : 1 : \sqrt{3} \qquad \text{A.}$$

the cube in its rhombohedral position. Striations parallel the terminal edges of the cube are characteristic of this form, and constitute an interesting proof for the correctness of this arrangement of the atoms in the crystal (Pyrites).

But we may also have aggregation simply in plane xz or xy, according to a *square;* hence either

$$x = z = \sqrt{3}$$

and the form

$$x : y : z = \sqrt{3} : 1 : \sqrt{3} \qquad \text{B.}$$

or we may have

$$x = y = 1$$

and the form

$$x : y : z = 1 : 1 : \sqrt{3} \qquad \text{C.}$$

It must not be forgotten that y in these forms has not the same *absolute* length; but we cannot go into any detail here, and must defer this point to some other occasion. (See also *Atomechanik*, p. 31–34.)

According to the most characteristic examples, we shall call the form A: *Pyrite-form*, B: *Rutile-form*, and C: *Quartz-form*. The axis y being always taken as unity, only x and z have the deviations ξ and ζ from the normal values, due to the weight, form, etc., of the atoms A and B.

The following table contains all minerals of the type AB_2 the crystalline form of which has been determined. For the sake of simplicity, I give in this table only the name, formula, crystalline system, *vertical prism*, *i.e.* the angle between the vertical prism ∞ P (for x vertical) and the axis y; the deviation ζ of the actual form from the normal; also the deviation ξ, the latter written in the column corresponding to the aggregation being according to *Quartz* (C), *Pyrites* (A) or *Rutile* (B). At the close of the paper will be found a table giving the observations and their sources; by means of a figure for each species, the reference is made easy.

Name.	Reference.	Formula.	Crystall. System.	Vertical Prism.	Deviation. $\zeta = x - \sqrt{3}$	Quartz, $x - 1$	Pyrites, $x - \sqrt{\frac{3}{2}}$	Rutile, $x - \sqrt{3}$
						ξ	ξ	ξ
I. R Fl.								
Fluorspar.	1	Ca $\left\{\begin{matrix}Fl\\Fl\end{matrix}\right.$	tesseral	60° 0′	0.000	. . .	0.000	. . .
Yttrocerite.	2	R $\left\{\begin{matrix}Fl\\Fl\end{matrix}\right.$	tesseral	60° 0′	0.000	. . .	0.000	. . .
Fluocerite.	3		tesseral?	60° 0′	0.000	. . .	0.000	. . .
II. RCl_2.								
Cotunnite.	4	Pb $\left\{\begin{matrix}Cl\\Cl\end{matrix}\right.$	prismatic	59°19′	−0.047	+0.003	. . .	. . .
III. $ƟR_2$.								
Copper glance.	5	S $\left\{\begin{matrix}Cu\\Cu\end{matrix}\right.$	prismatic	59°47′	−0.014	+0.111	. . .	. . .
Stromeyerite.	6	S $\left\{\begin{matrix}Cu\\Ag\end{matrix}\right.$	prismatic	59°47′	−0.014	?	. . .	. . .
Silver glance.	7	S $\left\{\begin{matrix}Ag\\Ag\end{matrix}\right.$	tesseral	60° 0′	0.000	. . .	0.000	. . .
Naumannite.	8	Se $\left\{\begin{matrix}Ag\\Ag\end{matrix}\right.$	tesseral	60° 0′	0.000	. . .	0.000	. . .
Hessite.	9	Te $\left\{\begin{matrix}Ag\\Ag\end{matrix}\right.$	tesseral?	60° 0′	0.000	. . .	0.000	. . .
Cuprite.	10	O $\left\{\begin{matrix}Cu\\Cu\end{matrix}\right.$	tesseral	60° 0′	0.000	. . .	0.000	. . .
Water.	11	O $\left\{\begin{matrix}H\\H\end{matrix}\right.$	hexagonal	60° 0′	0.000	?	?	?
IV. $ŔS_2$,		with *As*	replacing	S.				
Marcassite.	12	} Fe $\left\{\begin{matrix}S\\S\end{matrix}\right.$	prismatic	59°31′	−0.034	. . .	+0.049	. . .
Pyrite.	13		tesseral	60° 0′	0.000	. . .	0.000	. . .
Hauerite.	14	Mn $\left\{\begin{matrix}S\\S\end{matrix}\right.$	tesseral	60° 0′	0.000	. . .	0.000	. . .
Gersdorffite.	15	Ni $\left\{\begin{matrix}S\\As\end{matrix}\right.$	tesseral	60° 0′	0.000	. . .	0.000	. . .
Ullmannite.	16	Ni $\left\{\begin{matrix}S\\Sb\end{matrix}\right.$	tesseral	60° 0′	0.000	. . .	0.000	. . .
Cobaltine.	17	Co $\left\{\begin{matrix}S\\As\end{matrix}\right.$	tesseral	60° 0′	0.000	. . .	0.000	. . .
Smaltine.	18	(Co, Ni) $\left\{\begin{matrix}As\\As\end{matrix}\right.$	tesseral	60° 0′	0.000	. . .	0.000	. . .
Mispickel.	19	Fe $\left\{\begin{matrix}S\\As\end{matrix}\right.$	prismatic	60°23′	+0.027	−0.014	. . .	. . .
Leucopyrite.	20	Fe $\left\{\begin{matrix}As\\As\end{matrix}\right.$	prismatic }			. . .	. . .	. . .
Rammelsbergite.	21	Ni $\left\{\begin{matrix}As\\As\end{matrix}\right.$	prismatic }	isomorphous with Mispickel.		. . .	. . .	. . .
Glaucodote.	22	(Fe, Co) $\left\{\begin{matrix}S\\As\end{matrix}\right.$	prismatic }			. . .	. . .	. . .
Sylvanite.	23	(Ag, Au) $\left\{\begin{matrix}Te\\Te\end{matrix}\right.$	prismatic	60°33½′	+0.043	−0.034	. . .	. . .
Molybdenite.	24	Mo $\left\{\begin{matrix}S\\S\end{matrix}\right.$	hexagonal	60° 0′	0.000	?	?	?
V. RO_2.								
Cassiterite.	25	Sn $\left\{\begin{matrix}O\\O\end{matrix}\right.$	quadratic	56° 5′	−0.245	. . .	. . .	−0.245
Rutile.	26	Ti $\left\{\begin{matrix}O\\O\end{matrix}\right.$	quadratic	57°12½	−0.180	. . .	. . .	−0.180
Pyrolusite.	27	Mn $\left\{\begin{matrix}O\\O\end{matrix}\right.$	prismatic	57°12½	−0.180	+0.066	. . .	. . .
Zircon.	28	(Si, Zr) $\left\{\begin{matrix}O\\O\end{matrix}\right.$	quadratic	57°21′	−0.171	. . .	. . .	−0.171
Quartz.	29	Si $\left\{\begin{matrix}O\\O\end{matrix}\right.$	hexagonal	60° 0′	±0.090	+0.099	. . .	. . .
Anatase.	30	Ti $\left\{\begin{matrix}O\\O\end{matrix}\right.$	quadratic	60°34′	+0.045	± 0.000	. . .	. . .
Brookite.	31	Ti $\left\{\begin{matrix}O\\O\end{matrix}\right.$	prismatic	60°42′	+0.050	+0.059	. . .	. . .

A simple glance at the column of the vertical prism or the values of ζ is sufficient to show that these compounds really are hexagonal or nearly so, as required by my theory; in other words, *that the triangle AB_2 is equilateral or nearly so.*

We see also that all the orthogonal systems are represented; the tesseral 13 times, the quadratic 4 times, the hexagonal 3 times, and the prismatic 11 times: thus showing again the fallacy of certain systematists who cannot conceive of continuous variation, but see everywhere chasms and fictitious boundaries.

We have not time for a special consideration of the deviations ξ and ζ; but we cannot omit at least a few general remarks.

It is evident, that the cube is the most general position of equilibrium; indeed, if the atoms A and B were really equal material points, the cube would probably be the only form occurring in nature. We find this cube most perfectly represented in Pyrites and in Fluospar; in the former $Fe = 56$, $2\,S = 64$, in the latter $Ca = 40$, $2\,Fl = 38$, or A very nearly equal to 2B. Furthermore we notice, that the pyrites form, or $x = \sqrt{\tfrac{3}{2}}$ indeed is the most common.

Again, the form $x = y$, the square of the short axis is easier formed than the square of the long axis $x = z$; and accordingly we see this latter form only in 3 cases (Rutile, Cassiterite, and zircon) and even in these cases with the axis z very much shortened.* This variation can of course only be explained by reference to the composition of the elements; hence we must omit it here. But we may determine the rate of variation of ζ as dependent on the atomic weight Tτ of the titanoids Sn, Ti, Zr, Si entering in the composition; for zircon we must of course take the mean of Zr and Si, according to the formula of zircon. We obtain

	Tτ	$-\zeta$	calcul.
Cassiterite	118	0.245	0.248
Rutile	50	0.180	0.180
Zircon	$\frac{89.6 + 28}{2} = 59$	0.171	0.189

We see the last two correspond well enough. We may, for

* Also in *Tridymite* the form of SiO_2 discovered since this paper was written.

Rutile take 0.175, the mean, as the value corresponding to the atomic weight of 50. But $\frac{0.245-0.175}{118-50} = \frac{0.070}{68} = 0.001$ very nearly; hence

$$-\zeta = 0.130 + 0.001 . \text{T}\tau,$$

which gives the values above given as calculated, corresponding well enough to show that the deviation is principally proportional to the weight of the atom, *that is to the deviating mass, at the rate of* 0.001 *for each unit of the atomic weight of the element.* This formula would give for Pyrolusit the deviation $-\zeta = 0.185$ instead of 0.180 observed. For Quartz we would accordingly obtain a *quadratic* form, having $-\zeta = 0.158$ or $z = 1.574 = \text{tg}\, 57° 35'$; but this form has not yet been observed, the common form of Quartz being hexagonal, $\zeta = o$.

This rate of increase becomes of very considerable interest, when compared to that of other compounds. For the axis $z = \sqrt{3}$ we have the following deviations:

Sulphate-Series,	$-\zeta = 0.016 + 0.0008 . Xa.$
Aragonite-Series,	$\zeta = 0.163 + 0.0009 . Xa.$
Rutile-Series,	$-\zeta = 0.130 + 0.0010 . T\tau.$

or the variation of the same axis in these compounds is respectively

8, 9, 10 *ten-thousandths*

of the unit $y = 1$ for each unit in the atomic weight of the disturbing element, so that as a first approximation we have here, as in astronomy, the *disturbing force proportional to the disturbing weight or mass!*

In conclusion we must say one word in regard to the *sign* of the deviation. For the *cube* the sign of ζ and of ξ must necessarily be opposite; a contraction in one direction (—) must correspond to an expansion (+) in the other. We can test this on Marcassite, for which indeed ξ is positive, while ζ is negative. Since the cubical Pyrites has the same composition, so that the unit y for both minerals is the same absolute quantity, it follows that the atomic volume must be measured by x, y, z, or the crystallic volume; hence the *density* of these two modifications of $Fe\,S_2$ must be inversely as the product x, y, z, of their crystallographic axes. Now this product x, y, z, is

$$\frac{\text{Marcassite}}{\text{Pyrites}} = \frac{2.161}{2.121} = 1.02$$

while the density is

$$\frac{\text{Pyrites}}{\text{Marcassite}} = \frac{4.8 \text{ to } 5.0}{4.7 \text{ to } 4.8} = 1.02 \text{ to } 1.04$$

showing sufficient correspondence.

For the *other forms* the deviations must on the whole be towards the simplest position of equilibrium, that is toward the cube. Hence in the quartz form ξ must be positive, to make $y = 1$ approach toward $y = \sqrt{\frac{3}{2}}$; in the rutile-form it must be negative, for the same reason, y being $\sqrt{3}$ or greater than in the cube. By means of the table it will be seen that there are only two exceptions to this rule; the Mispickel group and Sylvanite having ξ negative, although they belong to the quartz-form, $x = 1$. But in each case it may be due to the presence of an associate element of high atomic weight: Gold ($Au = 197$) with silver ($Ag = 108$) in sylvanite, and Arsenic ($As = 75$) with sulphur ($S = 32$) in the Marcassite group.

So whether we look upon the crystalline form of the compounds of the water-type in general, or consider the deviations from the normal form in particular, we find the most perfect harmony between calculation and observation. But the more accurate calculation of these deviations can only be made upon the basis of the *composite nature of the elements*. As in astronomy it would be impossible to estimate the precession of the equinoxes and the nutation, if we would close our eyes to the fact that the earth is ellipsoidal and not a perfect sphere; so we cannot here calculate these crystallographic perturbations *unless* we admit the individual form and structure of the atoms of the elements, which according to my Atomechaniks is very far from spherical. But not only the crystalline form; also the *atomicity* of the elements require the aid of pantogen for a successful explanation.

APPENDIX.

TABLE OF THE OBSERVED VALUES MADE USE OF IN THIS ARTICLE.

No. of Species.	x.	y.	z.	Notes.
2	?	?	?	cleavage in 2 directions at angle of 108°30′.
4	2 a = 1.1906	c = 1.1868	2 b = 2.000	Prism. i Ī.
5	$\frac{2}{3}$ a = 1.1116	b = 1	a = 1.7176	
6	. . . .	. . . .	. . . .	isomorphous with No. 5.
12	b = 1	$\frac{a}{2}$ = 0.7868	c = 1.3287	Prism. $\frac{1}{2}$ Ī.
19	$\frac{2}{3}$ c = 0.9862	b = 1	a = 1.7588	Prism. 1 ī : 1 ī.
23	$\frac{2}{3}$ c = 0.9684	b = 1	a = 1.7721	Prism. 1 ī : 1 ī.
25	$\frac{1}{a}$ = 1.487	$\frac{a}{a}$ = 1	$\frac{1}{a}$ = 1.487	1 i : 1 i = 112°10′.
26	$\frac{1}{a}$ = 1.552	$\frac{a}{a}$ = 1	$\frac{1}{a}$ = 1.552	1 i : 1 i = 114°25′.
27	c = 1.066	b = 1	2 a = 1.552	
28	$\frac{1}{a}$ = 1.561	$\frac{a}{a}$ = 1	$\frac{1}{a}$ = 1.561	1 i : 1 i = 114°42′.
29	a = 1.0999	b = 1	b $\sqrt{3}$	0 : $\frac{1}{2}$ i = 150°42′.
30	c = 1	b = 1	a = 1.7723	1 i : 1 i = 121°8′.
31	a = 1	b = 1.05889	2 c = 1.78228	e of *v. Kokscharow*.

All these values, with the exception of the last (No. 31) are taken from *Dana's* Mineralogy, fourth edition. The hexagonal and particularly the tesseral forms are not here enumerated, since the system gives the numerical values.

The reading of this article was illustrated by numerous models and diagrams.

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24. A New and General Law Determining the Atomic Volume and Boiling Point of a Great Number of Carbon Compounds. By Professor Gustavus Hinrichs, Iowa State University.

From the atomic constitution of a very great number of organic compounds, especially the oxides of the hydrocarbons, I have deduced a very simple and general law of the *atomic volume;* this law being confirmed by observation, will accordingly be considered an additional proof for the correctness of the constitution of compounds as given in my *Atomechanik*, 1867. The new law for the atomic volume is:

Compounds of n carbon atoms, these being joined by pairs of *hydrogen-atoms and provided with a terminal water atom H_2O, possess the atomic volume*

$$v_n = 1 + n,$$

the atomic volume of water being taken as unity.

If a, the atomic weight ($O = 16$, $H = 1$, etc.), and s, the specific gravity observed, the resulting or OBSERVED

atomic volume is $v = \frac{a}{18.s}$ for any body.

In comparing these observed values v, with the values v_n, calculated by means of the above law, it must be remembered, that the atomic volumes are only strictly comparable at the boiling points; hence if the coefficient of expansion of the liquid be α (for each degree C) and the *difference* between the boiling point T and the temperature t at which the specific gravity s is determined be $\tau = T - t$, then the volume v at t would become

$$v' = v\,(1 + \alpha\,\tau)$$

In other words, the lower the temperature t at which s is determined below the boiling point of the liquid, the *greater* the increase which the volume would suffer by heating the substance to its boiling point. We shall now proceed to the consideration of some of the groups which come under the above law; and from these examples the law itself will become better understood.

I. The Monatomic Alcohols.

Their empirical formula is $C_nH_{2n+2}O$; their true constitution is represented by the longitudinal elevation of one atom:

$$\overset{o}{{}^{h}C}{}^{h}\,C^{h}\,C^{h}\,\ldots\ldots\,{}^{h}C^{h}\,C^{h}\,C^{h} \qquad (n \text{ atoms of } C)$$

where each h indicates the two atoms of hydrogen projected at the spot h. From this formula or figure it is apparent, what we mean by the pairs of hydrogen-atoms ($h = 2\,H$) tying together the carbon-atoms (C), and $h\,O = H_2O$ is seen as a "terminal" to the alcohol-atom. Whether now this is the true atomic constitution, or not, we may leave an open question here; it will be sufficient to show that the above law of atomic volume results from it, *and that experience does confirm this law!*

In this atom there are *n* equal terms C^h united in the same manner to form a prism; let the volume occupied by any one C^h be *x*, then the volume of the entire prism $C_n H_{2n}$ will be *n x*. The atom water ($H_2O = {}_hO$) when free has the volume = 1. Its volume in this liquid compound may therefore also be taken as *one*. The constitution of $C^h = CH_2$ and the terminal OH_2 is the same; it is therefore probable that the space they occupy is the same, or that $x = 1$. Hence the entire volume of the atom $C_nH_{2n+2}O$ will be $1 + n$ as above stated.

We now give the observed values of the specific gravity *s* the observed atomic volume *v* deduced therefrom; by comparing this with the calculated value v_n we determine the deviation *d* in per cent. of v_n. We finally add the boiling point T (in centigrades).

Alcohol,	*n*	*s*	*v*	v_n	*d*	*T*
Methyl,	1	0.81	2.18	2.00	+9	66°
Ethyl,	2	0.79	3.12	3.00	+4	78.4°
Butyl,	4	0.89	5.10	5.00	+2	109°
Propyl,	5	0.825	5.94	6.00	—1	132°
Octyl,	8	0.823	8.73	9.00	—3	180°

Since now *s* is determined at about the same temperature, this temperature is comparatively *near* the boiling point of the lower members (*n* small), but far below the boiling point for the higher members of the series. In other words, the determinations of *v* for the lower members is too high, as compared to that of the higher members. Indeed we see the deviations of observation minus calculation *positive* for the lower members, *negative* for the higher members, and regularly decreasing from the first to the last. By means of our formula, before given, we can readily see that the deviations must be of the form

$$d = h + k\mathrm{T};$$

and indeed by

$$d = 12 - 0.1\,\mathrm{T}$$

we get

n =	1	2	4	5	8
d observed =	+9	+4	+2	—1	—3
d calcul. =	+6	+4	+1	—1	—6

almost identic, or showing that our law $1 + n$ indeed is the true law for the atomic volume of the monatomic alcohols.

This influence of the temperature is also well shown in the case of ethyl-alcohol, C_2H_6O. At 4°C its specific gravity is 0.81, hence its atomic volume $v = 3.15$; but at 10° C its specific gravity 0.79 gives the volume 3.23, an increase of 0.08 for an increase of 6°C in temperature. Hence for a limited range of temperature the atomic volume of alcohol is at the temperature t equal to

$$v_t = 3\ [1.032 + 0.0044\,t]$$

or $v_t = 3$ exactly at about —7° C. This is accordingly 85° below its boiling point, so that if we adopt the general view that liquids may properly be compared at equal distances below their respective boiling points, the densities if determined at the following temperatures

	—71°	—7°	+24°	+47°	+95°
for n	1	2	4	5	8°

the resulting atomic volumes would be *exactly* equal to the theoretical values

2	3	5	6	9.

II. Acids of the Monatomic Alcohols.

The empirical formula of these acids is $C_n H_{2n} O_2$. They are derived from the alcohols by replacing the terminal $h = H_2$ by O; or

$$\overset{o}{{}^{h}}C^h\,C^h\,C^h \,.\,.\,.\,.\,.\, {}^{h}C^h\,C^h\,C^o \qquad (n \text{ atoms C})$$

The atomic volume is the *same* as for the corresponding alcohol.—We have:

Acid	n	s	v	v_n	d	T
Formic	1	1.235	2.07	2.00	+3	100°
Acetic	2	1.063	3.12	3.00	+4	118°
Propionic	3	0.991	4.12	4.00	+3	140°
Butyric	4	0.974	5.015	5.00	+0.3	162°
Valerianic	5	0.938	6.030	6.00	+0.5	174°
Capronic	6	0.931	6.916	7.00	—1.3	199°
Lauric	12	0.883	12.604	13.00	—3.3	?

These figures need no commentary; the remarks above made apply also in this case.

III. Different Compounds of equal Atomic Volume.

Quite a number of such serial compounds, of like atomic structure, but *very different empirical formula*, will accordingly have the same atomic volume, if only they contain the same number of carbon atoms. We shall consider several cases with 1, 2, 3 carbon atoms, and also some monamines.

A. $n = 1$; hence *atomic volume* $= 1 + n = 2$.

		s	*v*	v_n	*d*	*T*
Formic acid,	CH_2O_2	1.235	2.07	2.00	+3.5	100°C
Methyl alcohol,	CH_4O	0.814	2.18	2.00	+9	66°

The deviation for the latter, with a much lower boiling point, is much greater than for the former, in accordance with the preceding explanation.

B. $n = 2$; hence *atomic volume* $1 + n = 3$.

		s	*v*	v_n	*d*	*T*
Aldehyde	C_2H_4O	0.801	3.05	3.00	+1.6	21°
Glycol	$C_2H_6O_2$	1.125	3.06	3.00	+2	197.°5
Acetic acid	$C_2H_4O_2$	1.063	3.13	3.00	+4	117°
Alcohol	C_2H_6O	0.81	3.15	3.00	+5	79°

It appears from this that with the exception of aldehyde, all deviations are so much the smaller, the higher the boiling point, in conformity with preceding explanations. But the case of aldehyde does not represent any anomaly; for my theoretical formula is

$$\text{Aldehyde} \quad {}^{h}\overset{o}{C}{}^{h}\,C$$

or being devoid of the terminal $h = H_2$, and correspondingly this compound has at common temperatures a somewhat smaller atomic volume.

For the latter three it is seen that an increase of 40° C in the boiling point diminishes the volume about one per cent.; hence the deviation for aldehyde ought to be about + six per cent., while it really is only about + two per cent. Hence the removal of the terminal $H_2 = h$ corresponds to only about four per cent., or $\frac{1}{25}$ of the unit of volume, the volume of one atom of water.

C. $n = 3$; hence *the atomic volume* $= 4$.

		s	v	v_n	d
Glycerine	$C_3H_8O_3$	1.28	4.00	4.00	0.0
Lactic acid	$C_3H_6O_3$	1.215	4.11	4.00	—3
Propionic acid	$C_3H_6O_2$	0.991	4.15	4.00	+4

D. Monamines have the same mechanical structure as the alcohols, except that the terminal water H_2O is replaced by ammonia H_3N. Hence since $H_3 : H_2 = 3 : 2$, the former H^3 forming an equilateral triangle each side of which is nearly equal to the distance between H and H in water H_2O, it follows that the atomic volume of NH_3 must be determined by

$$H_3N : H_2O = 3 : 2 \text{ or vol. } H_3N = 1.5$$

and hence the volume of the monamines $C_n H_{2n+3} N = NH_3 + n\,CH_2$ must be

$$v_n = 1.5 + n$$

This is fully confirmed by the observations at my disposition; these give

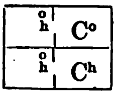
	n	s	v	v_n	d
Ammonia	0	0.62	1.52	1.50	+1
Ethylamine	2	0.696	3.59	3.50	+3
Amylamine	5	0.750	6.44	6.50	—1

The correspondence between calculation and observation is indeed most remarkably close.

E. Graphical Representation. Let the square represent one atom of (liquid) water, then according to the above, the following will be a graphic and geometrically true representation of the atomic volume and constitution of the compounds enumerated in D.

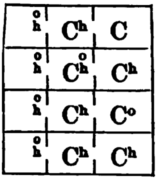
	Name.	Volume.
	Water	1.

$n = 1$



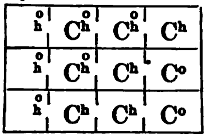
Name.	Volume.
Formic acid	2
Methyl alcohol	2

$n = 2$



Name.	Volume.
Aldehyde	3
Glycol	3
Acetic acid	3
Alcohol	3

$n = 3$



Name.	Volume.
Glycerine	4
Lactic acid	4
Propionic acid	4

This table might be extended; but we only shall observe here once more and explicitly, that the transverse section of all these compounds is the same, viz.,

O
H H
C

and hence the volume of this prism, or the atomic volume simply measured by $1 + n$. See *Atomechanik*, § 123, p. 18.

Even Sucrose, $C_{12} + 11\,(H_2O)$ gives the atomic volume $v = 11.83$ from the observed value of its specific gravity (1.606); now, since no H_2O is terminal, we may have according to the above principle, $v_n = n = 12$ which is only 0.17 or $1\frac{1}{2}$ per cent. greater than the observed value. There being no terminal water, the volume is not $1 + n$ but simply n.

Atomic Volume of the Elements.

According to *Atomechanics*, the constitution of the element-atoms is in so far similar to the constitution of the preceding carbon compounds that the element-atoms consist of a *series of plates* (called *atomares*) which may be compared to the consecutive links $CH_2 = C^h$ of the carbon series. A direct proof therefore we have in the circumstance that these *atomares* in the same group of elements occupy nearly the same volume, or are *isostere*.

If the atomic volume of an element be v (water $= 1$) and the number of *atomares* in the atom be m (*atometer*), then the volume of each atomare of this element will be

$$w=\frac{v}{m}.$$

Taking the values of m as given in my Atomechanics, we find for

PHOSPHOIDS Φ:

	v	m	w	F
P	0.94	9	0.10	44°
As	0.72	8	0.09	?
Sb	1.00	13	0.08	450°
Bi	1.14	12	0.09	265°

Here the influence of the fusing point F will fully account for the slight variation; w for *Sb* is the smallest, because its volume is taken farthest below its fusing point; that of P is the largest, because taken nearest its fusing point.

CHLOROIDS, X:

	v	m	w
Cl	1.48	13	0.113
Br	1.49	13	0.114
Io	1.42	12	0.118
mean			0.115

CADMOIDS, $K\delta$:

$v=p.K$

	obs.	*p.*	*calc.*	*diff.*
Mg	— 0.76	3	0.75	+0.01
Zn	0.51	2	0.50	+0.01
Cd	0.71	3	0.75	—0.04
Pb	1.00	4	1.00	0.00

The cadmoids accordingly follow the same law, a number p multiplied into the same constant, giving the atomic volume v inside the errors of observation. We may accordingly consider, by comparing this result with the law governing the carbon series, that one atom of

Zink	consists	of	2	atomares
Cadmium	"	"	3	"
Lead	"	"	4	"
Magnesium	"	"	3	"

which coincides with the formula given in atomechanics, viz.,

$Zn=A+B$	where	$A=2.4^2$ or [4.4]
$Cd=A+2B$	"	$B=[8.12]$
$Pb=A+3B$		
$Mg=3A$	"	$A=[4.4]$

so that vol. $A=$ vol. $B=0.25$ or $\frac{1}{4}$ of one atom of water.

KALOIDS, *Ka*:		v	p	*calc. v.*	*diff.*
	Li	0.65	1	0.63	+0.02
$v = p.K$	*Na*	1.32	2	1.26	+0.06
$K = 0.63$	*Ka*	2.50	4	2.52	—0.02
	Rb	3.11	5	3.15	—0.04

thus plainly indicating that the successive Kaloid atoms contain 1, 2, 4, 5 or q, $2q$, $4q$, $5q$ atomares. This result differs from the formula given in Atomechanik, §25. We may therefore conclude, that this preliminary formula, $Ka = 2\ (7) + 2\ m.\ 4^2$, given in Atomechanics is not the true formula, and does not quite correctly express the atomic constitution of the Kaloids. The values of m are respectively: *Li*, 0; *Na*, 1; *Ka*, 2; *Rb*, 5.

CONCLUSIONS.

The law for the atomic volume of serial compounds is very different from what chemists have been led to believe by *Kopp* and others; for we cannot, from the empirical formula, calculate the volume by giving a certain fixed value to each constituent atom, as *Kopp* has attempted, but the volume is simply that of the PRISMATIC ATOM or the product of maximum transverse-section into altitude or length of the atom. The section for the atoms here considered has been equal to that of a water atom; and the unit of length equal that of a water atom also, so that the volume of the compound atom, or

$$1 + n\cdot$$

is simply the *length* of the atom, that of water being 1. Thus the four different compounds given under III, *B*, have all the same atomic volume, and yet if we put $C_2H_4O = A$, we have empirically

$$\begin{aligned} &\text{Aldehyde} &&= A + \text{zero} \\ &\text{Glycol} &&= A + H_2O \\ &\text{Acetic acid} &&= A + O \\ &\text{Alcohol} &&= A + H_2 \end{aligned}$$

from which by *Kopp's* reasonings we would have to conclude that the atomic volume of water, H_2O is the same as that of oxygen O, or of two hydrogen H_2 and the same as nothing, hence that H_4O in A does not occupy any space, and that the volume is exclusively determined by carbon; but then this is manifestly wrong, since

vol. water $H_2O = 1$ and not zero!
vol. methyl-alcohol $CH_4O = 2$, hence $C = 2$ (if $H = O =$ zero)
vol. ethyl alcohol $C_2H_6O = 3$ and not $2 \times 2 = 4$

which would follow from the above. We therefore see that the current reasoning from merely empirical formula leads into a labyrinth of contradictions; it may not be presumptuous to say, that our theoretical formula, based upon the atomechanical constitution of compounds, have the criterion of truthfulness, both in the simplicity of the law and the perfect harmony between the same and observation.

And finally, since we find that precisely the same law of atomic volume governs the elements, the conclusion appears well founded, that the *element atoms also are prismatic bodies, built of lamellæ or atomares.* This lamellar constitution is now proved by 1° the atomic weight; 2° the refraction-equivalents of the molecular-physicist and crystallographer, *Schrauf;* 3° by the absorption of radiant heat in gases, simple and compound, measured by *Tyndall;* 4° boiling and fusing points; 8° length of crystallographic axes; 6° atomicity, besides the (7°) atomic volume here spoken of. For details on these points see my *Atomechanik*, 1867, and on 3° see my paper,—Atomechanics proved by *Tyndall's* Experiments in the American Journal of Mining, May 2, 1868.

And from all this we again affirm the composite nature of the so-called elements, and predict that they first will be decomposed into these lamella or atomares; finally the latter will be resolved into panatoms.

THE BOILING POINT OF THE ISOMERS $C_6H_{14}O$.

The general law in regard to the boiling point referred to in the title of this paper is *the boiling point depends on the form of the atom; the more symmetric the atom, the lower the boiling point; and the reverse.*

We shall now illustrate it first by the isomers of the empirical formula $C_6H_{14}O$; thereafter by the isomeric amines.

The compounds $C_6H_{14}O$ are monoxides of hexan C_6H_{14}, the constitution of which is

hexan	${}^{h}C^{h}\ C^{h}\ C^{h}\ C^{h}\ C^{h}\ C^{h}$	$\substack{*\\*}\,|\,\substack{*\\*}\,|\,\substack{*\\*}\,|\,\substack{*\\*}\,|\,\substack{*\\*}\,|\,\substack{*\\*}\,|\,\substack{*\\*}$
	side view.	horizontal projection.

In these figures *h* stands for H_2 at the spot where both H are projected; a star for H, a bar for C. Now each of the places *h* or H_2 can unite with the oxygen atom to form water H_2O. There are seven pairs of H or seven *h*, so that the oxygen atom may be united at seven different places. But two and two of these places produce the same atomic body, and hence the same compound. If the atom of hexan be represented by P_6 (protypoid of six carbon atoms) the *monoxide* may be represented by the symbol

$$_{m}P_{(6-m)}$$

if *m* indicate the number *h* to the left of the oxygen atom, and consequently $6-m$ the number to the right. But it is evident that

$$_{m}P_{(6-m)} \quad \text{and} \quad _{(6-m)}P_{m}$$

represent the same atoms seen from two different sides. Hence the seven monoxides

$$_{0}P_{6} \qquad _{1}P_{5} \qquad _{2}P_{4} \qquad _{3}P_{3} \qquad _{4}P_{2} \qquad _{5}P_{1} \qquad _{6}P_{0}$$

are only representing the following FOUR *really different isomers*, viz.,

$$_{0}P_{6} \qquad _{1}P_{5} \qquad _{2}P_{4} \qquad _{3}P_{3}$$

These four compounds, all corresponding to the empirical formula $C_6H_{14}O$ are represented in the following table by their *symbol*, vertical and horizontal projection, and the typical formula; and it will readily be seen that the latter results from my horizontal projection, by simply drawing a line obliquely through the oxygen atom.

Name.	*Symbol.*	PROJECTION. *vertical.*	PROJECTION. *horizontal.*	*Typical formula.*
Hexyl-Alcohol,	$_{0}P_{6}$	${}^{\overset{o}{h}}C^{h}\ C^{h}\ C^{h}\ C^{h}\ C^{h}\ C^{h}$	$\substack{*\\o\\*}\,|\,\substack{*\\*}\,|\,\substack{*\\*}\,|\,\substack{*\\*}\,|\,\substack{*\\*}\,|\,\substack{*\\*}\,|\,\substack{*\\*}$	$\left.\begin{matrix}C_6H_{13}\\H\end{matrix}\right\}O$
Methyl-Amyl-Ether,	$_{1}P_{5}$	${}^{h}C^{\overset{o}{h}}\ C^{h}\ C^{h}\ C^{h}\ C^{h}\ C^{h}$	$\substack{*\\*}\,|\,\substack{*\\o\\*}\,|\,\substack{*\\*}\,|\,\substack{*\\*}\,|\,\substack{*\\*}\,|\,\substack{*\\*}\,|\,\substack{*\\*}$	$\left.\begin{matrix}C_5H_{11}\\CH_3\end{matrix}\right\}O$

Name.	*Symbol.*	PROJECTION. *vertical.*	PROJECTION. *horizontal.*	*Typical formula.*
Ethyl-Butyl-Ether,	${}_2P_4$	${}^hC^h\,\overset{o}{C^h}\,C^h\,C^h\,C^h\,C^h$	$\begin{smallmatrix}*\\ \\ *\end{smallmatrix}\Big\vert\begin{smallmatrix}*\\ \\ *\end{smallmatrix}\Big\vert\begin{smallmatrix}*\\ o\\ *\end{smallmatrix}\Big\vert\begin{smallmatrix}*\\ \\ *\end{smallmatrix}\Big\vert\begin{smallmatrix}*\\ \\ *\end{smallmatrix}\Big\vert\begin{smallmatrix}*\\ \\ *\end{smallmatrix}\Big\vert\begin{smallmatrix}*\\ \\ *\end{smallmatrix}$	$\left.\begin{matrix}C_4H_9\\ C_2H_5\end{matrix}\right\} O$
Propyl-Ether,	${}_3P_3$	${}^hC^h\,C^h\,\overset{o}{C^h}\,C^h\,C^h\,C^h$	$\begin{smallmatrix}*\\ \\ *\end{smallmatrix}\Big\vert\begin{smallmatrix}*\\ \\ *\end{smallmatrix}\Big\vert\begin{smallmatrix}*\\ \\ *\end{smallmatrix}\Big\vert\begin{smallmatrix}*\\ o\\ *\end{smallmatrix}\Big\vert\begin{smallmatrix}*\\ \\ *\end{smallmatrix}\Big\vert\begin{smallmatrix}*\\ \\ *\end{smallmatrix}\Big\vert\begin{smallmatrix}*\\ \\ *\end{smallmatrix}$	$\left.\begin{matrix}C_3H_7\\ C_3H_7\end{matrix}\right\} O$

From these graphic representations of the mechanical constitution of the atoms it will readily be seen, that ${}_3P_3$ is the most symmetric atom of all; indeed it is perfectly symmetric, so that if we could suspend it by the oxygen atom, the whole atom would be perfectly balanced. Again the alcohol ${}_0P_6$ is the least symmetric of these four isomers.

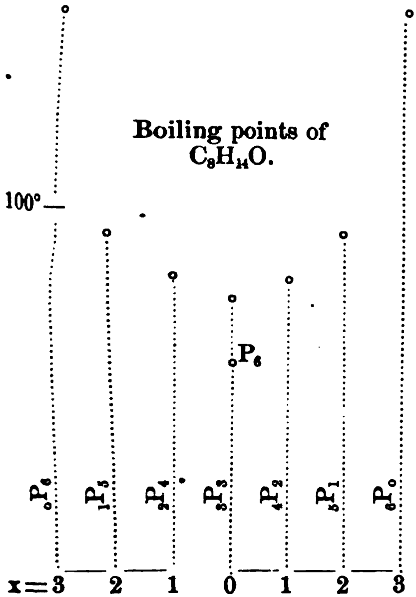
According to the law above given we conclude: *the boiling point of these isomers* $C_6H_{14}O$ *is* LOWEST *for* ${}_3P_3$, *highest for* ${}_0P_6$ *and increasing according to some definite law from* ${}_3P_3$ *to* ${}_0P_6$.

If x denote the abscissa, the distance from the central $h = H_2$, and t_x the corresponding boiling point (centigrade) we have indeed

$$t_x = 76^\circ - x + (4.25)^x$$

as the law expressing the variation of the boiling point for these isomers. That this law expresses the observed values we see from the following table:

Symbol	*x*	*t calc.*	*t obs.*
${}_0P_6$	3	149°.76	150°
${}_1P_5$	2	92°.06	92°
${}_2P_4$	1	79°.25	80°
${}_3P_3$	0	76°	76°



where the boiling point for ${}_3P_3$ has been determined by interpolation from the other simple ethers. These results are represented in the adjoining figure, on the scale of

$x = 1 = 1$ centimeter
$t = 1°C = \frac{1}{2}$ millimeter.

It is evident that a like law, but with constants depending on n corresponds to the isomeric compounds of any group of isomers expressed by

$$C_n H_{2n+2} O.$$

The Boiling Point of the Amines.

The amines are very rich in isomers and therefore eminently adapted to exemplify the law above given. By means of the horizontal projection we can readily represent the atomic constitution of the amines. They may all be considered as derivations of ammonia NH_3

$$\overset{\bullet}{\underset{\bullet \quad \bullet}{N}}$$

represented here in horizontal projection. The three sides HH may each grow by the addition of any number of $C^h = CH_2$, so that the general amine

$$\left.\begin{matrix} C_p H_{2p+1} \\ C_q H_{2q+1} \\ C_r H_{2r+1} \end{matrix}\right\} N \qquad \text{Symbol: } {}_p\overset{q}{N}_r$$

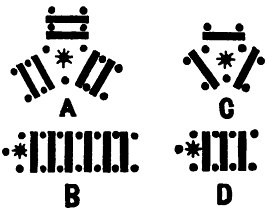
results by the prisms $C_pH_{2p} = p\,(CH_2)$, $q\,(CH_2)$ and $r\,(CH_2)$ being added on the three sides of NH_3 formed by the hydrogen atoms two and two. The symbol here given accordingly needs no farther explanation.

If now $p = q = r$, the amine is the *most symmetric* that can be formed by means of the same number of carbon atoms; it will, from among all the isomers represented by

$$C_{(p+q+r)}\,H_{2(p+q+r)+3}N$$

have the *lowest* boiling point.

In the adjoining figure



C = Trimethylamine — ${}_{1}\overset{1}{N}_{1}$

A = Triethylamine = ${}_{2}\overset{2}{N}_{2}$

B = Hexylamine = ${}_{0}\overset{0}{N}_{6}$

D = Propylamine = ${}_{0}\overset{0}{N}_{3}$

The observed boiling points confirm our law, as may be seen from the following table of *boiling points*, wherein isomers are in a horizontal line:

1	${}_{1}\overset{0}{N}_{0}$	below 0°				
2	${}_{2}\overset{0}{N}_{0}$	. . 18°.7	${}_{1}\overset{1}{N}_{0}$	. . . . 8°.5		
3	${}_{3}\overset{0}{N}_{0}$	. . 49°.7	${}_{2}\overset{1}{N}_{0}$	. . . . . ?	${}_{1}\overset{1}{N}_{1}$	. . . . 4°–5°
4	${}_{4}\overset{0}{N}_{0}$	. . 69°	${}_{2}\overset{2}{N}_{0}$	. . . . 57°.5	. . . .	
5	${}_{5}\overset{0}{N}_{0}$	. . 94°	. . . .		. . . .	
6	${}_{6}\overset{0}{N}_{0}$	. . 126°	. . . .		${}_{2}\overset{2}{N}_{2}$	. . . . . 91°
7	${}_{7}\overset{0}{N}_{0}$	. . 146°	. . . .		. . . .	
8	${}_{8}\overset{0}{N}_{0}$	. . 170°	. . . .		${}_{1}\overset{2}{N}_{5}$	. . . . 135°
9	. . . . . .		. . . .		${}_{2}\overset{2}{N}_{5}$	. . . . 154°
10	. . . . . .		${}_{5}\overset{5}{N}_{0}$	. . . . 170°	. . . .	
15	. . . . . .		. . . .		${}_{5}\overset{5}{N}_{5}$	. . . . 257°
	Monamines.		Diamines.		Triamines.	

This table needs no commentary. We see indeed that the more symmetric an atom, the lower is the boiling point; for

$_{2}\overset{0}{N}_{0}$	by	10°	greater than		$_{1}\overset{1}{N}_{0}$
$_{3}\overset{0}{N}_{0}$	by	44°	"	"	$_{1}\overset{1}{N}_{1}$
$_{6}\overset{0}{N}_{0}$	by	35°	"	"	$_{2}\overset{2}{N}_{2}$

According to the law

$$t_p = 25° p - 31°$$

expressing the boiling points of the monamines

$$_{p}\overset{0}{N}_{0}$$

with great accuracy, as may be seen from

p	*Name*	*Boiling point* *calcul.*	*observed.*
1	Methyl-amine	— 6°	below 0°
2	Ethyl "	+ 19	+ 18°.7
3	Propyl "	44°	49°.7
4	Butyl "	69°	69°
5	Amyl "	94°	94°
6	Hexyl "	119°	126°
7	Heptyl "	144°	146°
8	Octyl "	170°	170°

the boiling point of $_{15}\overset{0}{N}_{0}$ would be 344°, while the isomeric tri-amyl-amine $_{5}\overset{5}{N}_{5}$ is 257° or 87° lower!

Thus we may consider it a fact, that greater symmetry or equilibrium of an atom is accompanied with *lower* boiling point. The cause of this law is palpable. For when boiling, the liquid is converted into vapor of equal tension with the pressure on the liquid; but vapors and gases are known to be characterized by the atoms or particles moving equally in all directions. Now it will require a greater momentum, a greater power (hence a higher degree of a heat) to make the more unsymmetric body move equally well in all directions, than the more symmetrical body. Substituting heat for motion, and atom for body, we have our law in regard to the boiling point of isomeric bodies. — See also *Atomechanik.*

We have in this paper made use of deduction rather than of induction, because the method is much more lucid and satisfactory. The test remains the same: agreement with observation, and discovery of new laws and relations.

But if we had the opportunity we would herewith connect an *induction:* we would point out similar laws in the boiling and fusing points of the elements, and hence conclude their constitution to resemble that of the compounds here treated of. But since chemists have not yet realized that mathematics and observation must go hand in hand, in order to arrive at results comparable in universality to those worked out in astronomy and physics, it may be best not to push these conclusions at this time. If I have succeeded in showing that mechanical chemistry is a field as important and as grateful as any field now in scientific culture, I have attained my aim.

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25. Atomic Motions. By Professor H. F. Walling, of Easton, Pennsylvania.

(ABSTRACT.)

Commencing with the generally admitted proposition that all physical phenomena are made up of motions of matter, either "molar" or "molecular," new definitions of force and matter were advanced, drawing between them the line of demarcation more clearly and sharply than is done in the treatises on natural philosophy.

Matter was defined as *that which may be moved by becoming associated with force. Force as that which when associated with matter causes it to move.* We are justified in recognizing the substantive existence of force, as well as that of matter, and one object of this paper will be to show that the former may and does have an independent existence. Some of the so-called "primary properties" of matter, such as impenetrability, hardness, elasticity, etc., are not real properties of matter *per se*, but the phenomena upon which the supposition of their existence is based are manifestations of associated force and consequent motion.

Inertia is not a property of matter any farther than it expresses the principle that matter alone will not move. When matter and force become *associated*, the result is motion, and

since they do not spontaneously dissociate, this motion will be continuous. Opposing forces acting upon the same matter produce, if exactly equal and opposite, resultant rest.

The vexed question of the *infinite divisibility of matter* may be disposed of by admitting the mathematical principle upon which the differential calculus is founded, namely, that there are different orders of infinity both in the direction of immensity and of minuteness, the relations of quantities in the different orders being those which are considered to exist between finite and infinite quantities and between differentials of different orders. Thus chemical atoms, though infinitesimal when compared with finite bodies, have definite relative magnitudes when compared with each other, and may even be considered as infinitely great, when their diameters are compared with those of the lines of force by which they are traversed.

The postulates of the hypothesis now offered are,—

First: Force is omnipresent, occupying every possible straight line in both of its opposite directions.

Second: It is infinite in quantity in each and every line and direction.

Third: The amount of force which becomes associated with a given atom in a unit of time bears a constant ratio to the entire amount of force which acts upon the atom.

Fourth: When two atoms are situated in the same line of force there is an inequality of action producing results analogous to those which would follow the *interception* of a stream of matter or a succession of waves. The amount of force acting upon the second atom in the order of the force's direction, is less than that acting upon the first by an amount which will depend upon the magnitude or associating capacity of the latter. The word *interception* will, for convenience, be used hereafter to indicate the action here supposed.

Admitting these postulates, it will be seen that a single atom in space would be acted upon by equal forces in all directions and no motion would result. If, however, a second atom be introduced, mutual interception of force takes place in accordance with the fourth postulate, and resultant forces become mutually and simultaneously associated, urging the atoms towards each other.

The amount of force thus associated is always necessarily equal and opposite in the two atoms, for if we indicate the two atoms by A and B, and the amounts of force intercepted by each, respectively, in a unit of time from the external and opposite lines of force, by a and b, we have the resultant impelling A towards B,

$$a + \infty - (\infty - b) = a + b$$

while that impelling B towards A is

$$b + \infty - (\infty - a) = b + a$$

The law of the inverse squares is an obvious consequence of the assumption that atoms have definite size when compared with lines of force, being the law of radiant forces. For if the atoms are capable of subtending angles made by force lines, these angles will vary with the distances, and the integral of all the force rays which may pass through one atom to any point upon the other will be inversely as the square of the distance between the atoms.*

While this hypothesis of the cause of gravitation obviates the necessity for attributing an inherent attractive power to matter, enabling each atom to exercise an incredibly wonderful power of discriminating the positions of other atoms and of acting "where it is not," it likewise removes a most serious difficulty in reconciling the conservation theory with the facts in regard to gravitation. This difficulty cannot be better stated than in the words of Faraday, as follows: "There is one wonderful condition of matter, perhaps its *only* true indication, namely *inertia*, but in relation to the ordinary definition of gravity, it only adds to the difficulty. For if we consider two particles of matter at a certain distance apart, attracting each other under the power of gravity, and free to approach, they will approach; and when at only half the distance, each will have had stored up in it, because of its *inertia*, a certain amount of mechanical force. This must be due to the force

*This view of the cause of gravitation was advanced by the writer in a contribution to Silliman's Journal in 1864, vol. xl, p. 254. It was not until I read a very interesting paper by James Croll, in the "London Edinburgh and Dublin Philosophical Magazine," December, 1867, that I became aware that somewhat similar views had been previously entertained by Faraday, Waterston and others.

exerted, and if the conservation principle be true must have consumed an equivalent proportion of the cause of attraction, and yet according to the definition of gravity, the attractive force is not diminished thereby, but increased fourfold, the force growing up within itself the more rapidly, the more it is occupied in producing other force.

On the other hand if mechanical force from without be used to separate the particles to twice their distance, this force is not stored up in momentum or by *inertia*, but disappears; and three-fourths of the attractive force at first disappears with it. *How* can this be?"

I apprehend that the principle of conservation of energy, if applied only to *associated* force, that is, to the aggregate momentum or *vis viva* of matter in mechanical or molecular motion, can only be maintained by including in the estimate of *vis viva* an imaginary quantity called "potential energy," a conception which seems to me liable to every objection which can be brought against that of the independent existence of force, without the simplicity of the latter.

The harmony of my hypothesis with the conservation of force is evident. The equal and opposite forces which are stored up in the approaching bodies are taken from the lines of force in which the bodies are located, diminishing their quantities by just the amount which becomes associated with the bodies. When gravitation is counteracted by some other force and the approaching bodies are stopped and separated, the associated force is dissociated and restored to the line of force which is traversed.

An immensely important principle, a corollary to that just stated and forming one of the fundamental laws or axioms of dynamics, is here developed. Assuming, as I do, that the universal force already described is the only force in nature, it follows that no two atoms or bodies in the universe can, by mutual action, approach each other, without, by such mutual action, storing up equal amounts of force in opposite directions, nor can they separate without losing or imparting equal amounts of their associated force to the traversed line of force. When followed out to its results in all directions we shall perceive that this principle is equivalent to, or rather identical with, the "third law" of Newton, namely, that action and reaction are equal and opposite.

In order to see this more clearly it will be necessary to consider the nature of what are called *repulsions*. There is no more ground for maintaining the existence of *inherent repulsions* than of *inherent attractions*. I will now attempt, therefore, to show how bodies or atoms are impelled *from* each other.

The obvious and simple cause of this *apparent* repulsion is the momentum or *inertia*, of the matter in motion. A comet for example, whirls with almost incredible velocity around the sun, and flies off into space in a nearly parabolic orbit. Is repulsion the cause of this? No more than it is the cause of the continued ascending motion of the pendulum after it has passed its lowest point. The true cause in both cases is the momentum of the moving body. This is familiar to every student of astronomy or mechanics.

By applying the same principle to the motions of atoms we shall see that the ultimate or limiting force with which atoms or molecules will separate from each other is equal, in any pair, to the excess, at any position, of the actual momentum over that which would have been acquired in falling by mutual gravity, towards each other to that position from an infinite distance. This excess constitutes, therefore, the force of virtual repulsion.

If now we free our minds from the dogmas of the impenetrability of matter, hardness of atoms, etc., we may obtain wonderfully simple and satisfactory views of the constitution of matter, the transformations it undergoes and the function of heat in effecting these transformations.

Let us first consider the nature of heat, the cause of its "repulsive agency," and the reasons for the changes of condition from the solid to the liquid and from the liquid to the gaseous condition, which continued additions of heat produce upon matter.

If we suppose the atoms of all bodies to be in motion among each other, impelled towards each other by mutual gravitation, and moving beyond each other in accordance with the law of *inertia*, we may give the name heat to the atomic motion, and temperature will be represented by the momentum, or MV. We shall see that there may be three conditions of motion in a body or aggregation of atoms.

First: That in which the acquired motion is less than the potential motion of gravity; that is, less than would be acquired by falling from an infinite distance. In this case the orbits of the atoms would be such as to retain the aggregation within finite limits, corresponding to elliptical orbits of the solar system. This would be the *solid* condition of matter.

Second: That in which the acquired motion is just equal to the potential of gravity, or what would be acquired by falling an infinite distance. The "attractions" and "repulsions" would, in this condition be exactly balanced and the condition one of unstable equilibrium, represented by the surface of a volatile *liquid*, where, doubtless, vaporization and condensation are continually going on, as determined by minute variations of temperature and pressure.

Third: That in which the acquired motion exceeds the potential of gravity, when virtual repulsion takes place; that is, the atoms tend to separate to an infinite distance with a positive force, giving rise to the *gaseous* condition.

The problem of tracing deductively the paths of innumerable atoms, each attracting all the others according to the law of the inverse squares, seems one of hopeless complexity, and probably exceeds the power of the human intellect. It is only by inductive processes that we may hope to arrive at useful results in this direction. The problem is simplified very greatly by the chemical law of the absolute equality of weight or *inertia* in the atoms or molecules of homogenous substances. Moreover in gases, liquids and chemically homogeneous solids we must conclude that under a uniform temperature, towards which there is a constant tendency, absolute symmetry of atomic arrangement and motion must obtain, since in every portion, of whatever form or dimensions, we find precisely the same specific gravity, that is, equal numbers of atoms in equal spaces. Still farther, when gases are brought into contact with different gases, liquids with liquids (except when immiscible), and even when certain liquids and solids are mixed, a diffusion or solution takes place and the two or more subthe side containing the oxygen would be swelled to a larger bulk; the tendency to symmetrical distribution would at length assert itself however, and the two gases would be found mixed in equal quantities on each side of the septum.

When change of gaseous volume is produced by chemical union, we must suppose that the chemically combined atoms arrange themselves into systems, each of which, so far as heat motion or the mutual action of bodies to produce equilibrium of temperature is concerned, plays the part of a single atom.

In the hydrochloric acid type, for example, where the gaseous bulk remains unchanged, we infer that each atom plays its own role in heat operations. In the water type the single atom of oxygen plays one part, and the two of hydrogen another; in the ammonia type, the single atom of nitrogen plays one part, and the three of hydrogen another, and so on throughout all the infinite variety of chemical combinations. The volume of the compound gas, could we obtain the combination in a gaseous form, would accordingly determine into how many systems, each equivalent to a single atom in its heat functions, the combined atoms had aggregated themselves.

The development of heat by the compression of gases and its absorption by their expansion is another evident consequence of this hypothesis. By compression the atoms are brought under a more powerful gravitating influence, which increases the momentum, or, in other words, the temperature of the gas. In expansion exactly the reverse of this takes place; the atoms being withdrawn to a greater mean distance from each other are less reënforced by gravity, and their *momenta* or temperatures are reduced.

The remarkable law discovered by Dulong and Petit, that the specific heat of elementary bodies is inversely as their atomic weights, may be considered as direct evidence in support of the hypothesis that temperature is momentum. For if we admit this hypothesis, remembering that the measure of specific heat is not one of absolute associated force or momentum, but of *vis viva* or *work done* in increasing the velocity of the atoms, a measure of *space effects* produced or to be produced, it obviously follows that more of this kind of quantity will be required to produce the same momentum in a light atom than in a heavy one, in the exact ratio of the greater velocity imparted, or in the inverse ratio of the weights. This is just what Dulong and Petit have, by careful experiment, established in regard to many substances, both simple and compound.

In tracing the transformations of matter from the gaseous to the liquid, and from the liquid to the solid condition, we cannot be guided by *a priori* or mathematical investigations, and can only suppose such changes to take place in the paths and motions of the atoms, as, while conforming to the general principles already stated, will most fully coincide with the actual facts in the results to be expected from the theory.

We may, for example, suppose that in passing from the vaporous or gaseous to the liquid condition, the momentum of the atoms falls below the potential of gravity, so that instead of moving in continuous directions, the lines of atoms break up into closed orbits which are long ellipses or loops in form. They may be *interlinked* at their vertices in one direction in each plane.

At the moment when this occurs some remarkable changes take place. The atoms drop down upon each other from the distances due to their previous direct motions and the virtual repulsion of their surplus momentum, to that which will give equilibrium to their new closed orbits. The entire distance thus passed through of course depends upon the pressure to which the gas or vapor was subjected before it became condensed to a liquid. Thus water is about seventeen hundred times more dense than its vapor when the latter is confined at a pressure of fifteen pounds to the square inch.

All the atoms in a single orbit probably have now a single direction of angular motion, and stretch around in comparatively close proximity to each other, somewhat as a meteoric ring may be supposed to pass around the sun.

A reënforcement of gravity is produced by this approach of the atoms, assisted perhaps by the interlinking of the orbits, which reënforcement is accompanied by an acceleration of motion and consequent increase of temperature, or development of "latent heat."

Some partial idea of the immense quantity of force associated with the atoms of a liquid, and their consequently enormous velocity may be obtained by considering what amount of velocity would be generated by the force which would be required to raise a quantity of water, for example, one degree in temperature. This velocity for water would equal that generated in a fall of seven hundred and seventy-two feet, namely, somewhat less than 7×32 or two hundred and twenty-four feet in a second. If now the temperature of the water above absolute zero is, say four hundred degrees, we have a velocity of $\sqrt{400 \times 224}$ or four thousand four hundred and eighty feet in a second. It is not at all wonderful then, that water and other liquids resist compression, with such enormous force for any compression must still farther increase the velocity of the atoms and their consequent momentum and power to resist pressure. Each atom will be impelled against the compressing surface (or around its exterior atoms) with a velocity more than twice greater than that of the swiftest cannon ball.

In the liquid, as in the gaseous or vaporous condition, we find that the atoms or molecules are free to obey the impulses of any external force so long as it does not tend to reduce the amplitude of the periodical motion, or what is the same thing to compress the liquid into smaller bulk.

If we investigate the effect of a still farther diminution in the heat or velocity of atoms, we shall probably find that the long, elliptical, or loop-like orbits of liquids become gradually somewhat reduced in size and changed in form, so that at a certain period, a second or *lateral interlinking*, at the extremities of the conjugate axes or diameters, takes place, and a form of molecule is originated which gives the rigidity of structure found in solid bodies.

Doubtless the most direct path to positive knowledge in regard to the forms of solid molecules, is through the study of crystalline forms and forces. It is now generally understood that cohesive attraction in homogeneous substances is identical with the polar force of crystallization. A glance at the general features of crystallogeny, as now taught, will be likely, therefore, to render essential aid in the investigation.

Professor Dana, in the introduction to his great work on mineralogy, reduces all the various forms of crystals to primary ellipsoids, which he supposes to be held together by a powerful attractive force which suddenly comes into existence at the instant when crystallization takes place, at the extremities of conjugate axes of the ellipsoids.

While this hypothesis is amply sufficient to account for all the various *forms* of crystals, which it does in a simple and beautiful manner, the *source* of the polar force, by which the previously liquid molecules are compacted into the hard unyielding crystal, is left as much in the dark as before, and such questions as the following naturally occur to the student. What is the nature and constitution of this ellipsoid? Is it solid, hard, elastic, etc.? Why does the polar force remain dormant in the liquid, and suddenly start into existence at the instant of solidification? Why is it confined to six points on the surface of the ellipsoid? To these inquiries, the most diligent questioners of nature seem as yet to have received no satisfactory response.

In answer to such of them as have not already been answered in this paper I venture to make the following suggestions.

Upon the surface of the primary ellipsoid of Wollaston and Dana, suppose three ellipses to be described by the intersection of bisecting planes parallel with the sides of the circumscribing prism and to the planes of crystallization. These three ellipses will intersect each other at the six poles of crystalline attraction. Removing the ellipsoid, we will retain only the imaginary ellipses, which we will suppose to be replaced by the rings of atoms already described.

Solid molecules as before described are each made up of at least three rings, revolving with enormous rapidity in different planes, and intersecting each other at the six crystalline poles, each ring interlinking at four of these intersections with the corresponding ring of the adjacent molecule, thus calling into existence at the six poles that powerful attraction which was to be accounted for. Moreover the polar property of this attraction, which has heretofore effectually baffled its investigators, is clearly accounted for, being due to the *direction* of the planes of atomic motion, and the tendency to preserve these planes, and continue them from molecule to molecule.

To account for the ellipsoidal forms of the molecules, we have only to bear in mind that in a body of uniform temperature the mean *momenta* of the atoms are equal. If, therefore, the rings forming a molecule are unequal in weight it follows that the heavier rings will move in correspondingly shorter orbits.

In the case of chemical compounds, these rings as already suggested, might be made up of secondary systems analogous to those of planets and satellites in the solar system. These systems would most likely be so made up as to introduce no exaggerated and unstable disproportion into the dimensions of the elliptical rings forming the molecules.

Supposing each of these rings to be made up of similarly combined systems, the resulting form of molecule would be that of the sphere, the primary form of the monometric system of crystals. It is easy to see how all the other primary forms of crystals may be derived, by introducing rings of various lengths into the primary molecules.

It seems not improbable that, by carefully studying the modes of arrangement into systems and rings, the calculation of crystalline forms may be reduced to a problem in dynamics.

The limits of this paper will admit only a hasty glance at the hypothesis of the *universal ether*, now generally supposed to occupy celestial spaces, and to constitute the medium by which the light and heat of the heavenly bodies is transmitted with such wonderful velocity. If we admit the existence of such an ether, there seems to be no reason why it should be considered so different in its nature from other matter as to require a different definition or different laws of association with force. It is only necessary to remember that its atoms are relatively almost infinitesimal when compared with ordinary chemical atoms, since they have, so far, effectually eluded all attempts to weigh them, either by their retarding effects upon celestial motions, or by their incorporation into any chemical compounds. Hence in accordance with the dynamical principles already stated, they must move with an almost infinite velocity when compared with that of even the lightest ordinary known atoms, as, for example, those of hydrogen. This supposition seems to accord with the almost inconceivably rapid transmission of light and heat. When we have accurately determined the temperature of the celestial spaces and other necessary data, we may calculate the chemical equivalents of the etherial atoms by dividing this temperature by their velocity. Thus, if we estimate the velocity of a hydrogen atom, at a temperature of four hundred degrees above absolute zero, to be three miles per second, and the velocity of the etherial atoms, at one hundred and ninety-six degrees above absolute zero, to be one hundred and ninety thousand miles per second, we obtain the velocity of the latter at four hundred degrees by the following proportion, $\sqrt{196} : \sqrt{400} :: 190000 : 270000$ nearly, hence the chemical equivalent of the etherial atom, reckoning hydrogen at unity, is $\frac{3}{270000}$ or .000001 nearly.

In settlement therefore of the long controversy between the undulatory and corpuscular theories of light it may be found that both are partly right and partly wrong. That is, the optical effect is not produced by a direct impact of particles as supposed by Newton, but by transmitted disturbances of the dynamical equilibrium, in other words, by *vibrations*, while on the other hand the rapidity of the transmission is due to the proper motions of the atoms.

With this outline sketch, which has already occupied too much of your time, I leave the subject for the present. I cannot doubt that the application of this hypothesis to chemical, optical, and electrical science, will furnish a clue to some of their hitherto unsolved mysteries, and especially in the elucidation of that great principle of universal polarity or simultaneous exhibition of equal and opposite forces, for which no adequate cause has yet been produced. Ampere's beautiful theory of the identity of electricity and magnetism, for example, seems to be a confirmation of the hypothesis I have advanced. Indeed, the figures with which he illustrates the currents of electricity in magnets, seem to be drawn almost on purpose to indicate such atomic motions as I have been describing.

26. The Volcanic Tide Belt and the World's Flood-Gates. By Theodore C. Hilgard, M. D., St. Louis, Mo.

It is now the time to call attention to the fact that all large open sea-expanses (exclusive of all the tideless inland seas, such as the Baltic, the Mediterranean, Caspian, and our Lakes) are actually underlaid by cold strata, at a rapid rate deprived of heat, by the convection, at large, of polar undercurrents, often a great deal below the freezing point of fresh-water or ice, and by miles of hydrostatic height—and thus are notoriously of a much lower temperature than the adjacent continents at corresponding depths. Therefore, the large oceanic depths being more uniform, and at the least by as much thicker than the former, as the height of snow-line plateaux of continents is above the sea, and being accordingly of a specific density thereto corresponding, it follows that the crust of rock underlying the oceanic hemisphere, and the Arctic Basin, likewise, must represent a submarine impending inverted continent whereof the refrigerated, semirigid, basal mass is sealing down, as it were, by the semifused volcanic substances, our central bottomless pit of unquenched fires, with a liquid centre, and only incarcerated from above. Now the rigid crust under the sea is far more ponderous than that of the low continental expanses at corresponding depths, and often probably protrudes a jutting broadside, downward into the volcanic interior.

Nor is the geographical relation of volcanic action as yet fully appreciated in geophysical science.

The "continental" hemisphere, actually centering near the prophetic queen city of the "orbis terrarum" (or terraneous semiglobe) is a dome, or terrestrial cupola, the centre whereof as a pantheon's* "sky-light" is perforated by Mounts Etna, Stromboli and Vesuvius, etc., and is divided from a preponderatingly "aquatic" or "oceanic" one by an imaginary dividing-line such as, *e.g.*, claimed by Humboldt and other geographers; but which, if laid only ten degrees farther north, at its northern apex, in the Pacific Ocean, while actually containing as much oceanic expanse, when coinciding with the Aleu-

* or pandemonium's ?

tian Archipelago will thus at once be found identical with the *volcanic belt of the Globe* as a "greatest circle," almost undeviating from a girth cutting the equator at Sumatra (or in the explosive East Indian volcanic Archipelagoes) and likewise antipodally explosive in the equatorial precinct of the late disastrous earthquake; the famous province of Quito, with the Cayambe at latitude 0°, and in the immediate neighborhood of the clustering cones of Chimborazo, Cotopaxi, Antisana, Pichincha* and the volcano of Pasto farther north.

It must be recollected that this volcanic girdle forms an unbroken continuity, from the East Indies, or the Sunda Archipelago, through the Philippines, Loo-choo, the Japanese and the Kuriles Archipelagoes, and all the length of Kamtschatka, tending in a north-easterly direction, where the famous Aleutian Archipelago joins at right angles, and, together with the Peninsula of Alaska, forms nearly a quadrant of a circle around a point, almost within Behring Strait, and exactly on the Arctic circle, 23½° south of the present North Pole; through Alaska, British Columbia, Oregon, and almost striking the South Pass—that original crest of head-waters diverging into four important rivers, the Columbia, the Colorado, the Missouri and the Platte rivers—on through California, Mexico, Quito, Peru, Bolivia and Chili, this eruptive crack of our "earthen" Globe; switching off rather to the right, to Cape Horn, is thence dotted out by a rectangularly incident linear Archipelago composed of the Falkland Islands, the Aurora Islands, New Georgia, Brown's Islands and Sandwich "Land" (not "Islands"), whence, after a short interruption—by only one-fifth of a girth—the line, swerving to the left, subsumes Madagascar, the so-called Mascarenes, the Seychelles, the volcanic straits of Babel Mandeb, the Laccadives and Maldives, rejoining the line of direction only at Sumatra, rather along the outlines of all the adjacent continents.

Although only an approximative girth of our world, it emphatically separates the aqueous hemisphere from the "terrestrial," so-called one, by a succession of eruptive fissures, almost invariably following the *crude configurations of continents always near the coasts.*

*Pichincha alone stands as the apex of a *crest*, the rest are "cones."

On the Asiatic side they follow the coast-line far out at sea. On the American side they follow the coast rather closely in the land. The West Indies, however, are predicable as belonging to the great equatorial explosive intersection-ground, and are a probably "recent demersion" with the late elevation of Quito for a paragon. In either case they stand eastward of the border of the Pacific adjoining.

It is evident, as Professor Perrey of Dijon has repeatedly mooted, that a Plutonic semiliquid or liquid molten mass must obey the Newtonian tide-agencies more promptly than does the sea; the latter being only a few miles in depth, against a radius of four thousand miles of unaccounted, but partially liquid, nature. Of course, then "the planetary elongation" wrought by the computed gravitative velocitarian difference between our Globe's centre and its zenith, as well as nadir, and that towards and under the moon amounting to about five feet, would mainly occur in the semiliquid or fused central mass. The confining crust for continents is computed to have about twenty-five miles thickness; but it must in high seas — of several miles depth and of very low profundal temperatures — be estimated a good deal more rigid, resistant and uniform, and at the same time projecting below the terraneous crust by the entire depth of the ocean, and to which must be added the excess of thickness by the incident rigor, or huge abstraction of heat by contact with, and convection of, gelid submarine torrents of immense thickness or height as propelled from either pole towards equatorial coasts, and found under-running and partly opposing, partly favoring, the circular sea-currents as they move toward — and then return from — high latitudes: and whereby the coasts of South America receive from either pole a cold undercurrent, naturally conveyed through cortical channels, depressed by their own specific weight being increased by cold temperature, and thus likewise agglomerative of more (congelative) "basaltic" masses, *thereby sinking all cold channels and cold sea-grounds below the level of the continental crusts:* and it is *this* feature which at once explains and subsumes all the phenomena intimated in the heading. All the islands of the Pacific have only a slight tide, whereas there the hydrographic features would be most favorable to the formation of the superficial oceanic-sheet tide, which at the best could amount to only a fraction of an inch; whereas a uniformly and hydrostatically buoyant interior would heave up throughout (causing only a change in curve and in star-times). Besides, the fact shows all the tides to have the strongest power near the shores, and to be travelling waves, as notoriously admitted by all experts, with a velocity from east to west of at most three hundred to six hundred miles an hour; whereas the planetary rotation and coincident phenomena at the equator travel at the (rotatorial) velocity of one thousand miles an hour; hence the tide cannot be transmitted as a travelling wave! Whewell's well-reputed tide-chart in fact shows the *ebb to be directly under the culminating moon* at the five controllable points at the coasts, *near the equator.*

At Cape Coast Castle (longitude of Greenwich, with culminating moon at midnight), we find No. VI, which means tide at 6 o'clock, A. M.; hence, logically, ebb at midnight — or noon! Under the culminating moon thirty degrees or two hours farther west, at the coast of Brazil, we find VIII, which means ebb at 2, A. M. At a quadrant's distance we find the moon over the Gallopagos Islands, and full ebb from Lima to Guayaquil; and antipodally to Cape Coast Castle, we find likewise highest ebb at the Gilbert's Archipelago; and antipodally to Quito a full ebb at Sumatra, Ceylon, and Cape Comorin, all under the culminating moon! Now this is the true event and explains itself fully on the Newtonian premises of planetary elongative stress, but only under this condition that, *the rigid crust resisting*, the main stress results as a local *sublevatory shock or ebb* at the lines of least resistance, the warm and lowland coasts, causing a wave to roll off, toward the sea, which is not so pliant in its undercrust, and returning when the land is sunk, as the local *tide-roll!*

This fully explains all the tide-wave reverberations and retardations so aptly shown by a glance at Whewell's chart: both sides of South America, for example; while still the exciting cause, *i.e.* the sublevatory ebb-shock, unimpeachably performs its "underground railroad" motion after the well-known rhythm of a tide — or ebb — twice in twenty-five hours, as required by the positive fact of observation.

A system of five synchronous tides, and as exhibited by Whewell's Chart, thus becomes at once explicable, although one of them is at least a very dubious and at all events a supernumerary one on the supposition of a tide-roll itself performing the transit. It is the dodecad of intercalary sea-rolls inscribed between Adelaide and the Cape, whereas the text requires, instead of twelve hours, but one single hour's difference. A triad of synchronous wave-rolls would produce a tide three times in twenty-five, or about once every eight hours; which is contrary to the established rule.

As we can only rule a tide or ebb where there is land, the high-sea rolls escaping observation, all facts agree with the assumption of a subterraneous upheaval or ebb travelling once in twenty-five hours around the Globe. At the mouth of the Amazon we find a "bore," or rocker (hence "Pororocca") at Para, just before high tide. At ebb the moon is culminating there—*fide* Whewell. As the roll of the tide, cast off north-eastward, returns south-westward, and while the moon is already deeply sunk behind the Pacific Ocean, both the local tide and the slow African roll coming in by this time across the narrow Atlantic, this "bore" is produced. Again, the highest tide at the coast of Lima, etc., is when the action of the moon reverberates from the deep, cold, jutting bottom of the Pacific against the hot and thin coast, backed in the rear by the "heart of the Andes," so that the regurgitation swells the ebb to its highest. So is the high tide of all the Pacific shores due to the translatory under-rush recoiling from the Pacific bed as high ebb, and the tide afterwards rolling in broadside from the Pacific. The "bore" of the Hoogly or Ganges is evidently produced by the recoiling wave from the equatorial parts being narrowed in within the triangular Gulf of Bengal; and the Himalaya Mountains (close in the rear of the Ganges Deltas), strongly opposing the upheaval, the resultant relative depression in the rear actually diminishes the head or fall of the entire river-length sloping along the mountain-chain; and so the constricted northward roll finds a diminished head and not a backing up of rear waters, and hence the bore results.

As to the Maelstrom, it receives the gulf-current's Arctic outrunner, through the only channel half a mile deep, left between the Shetland and the Faroe Islands, in that grand submarine shoal or dyke of one-fourth of a mile in depth all the way from Scotland to Iceland, and all included in that tract which, according to Humboldt, is in a slowly rising condition. Now, every check of circulation of tropical waters towards polar ones, and in the case of the Arctic Ocean, every interception of the Asiatic or Atlantic gulf-currents may slowly eventuate in a perfect glaciation of the Arctic Sea and its adjacent countries!

Occasionally the diurnal sublunar coast-impact from beneath rupturing the coast under ground may thus give access to either marine or (heated) artesian waters, often of more than explosive temperatures—as manifest in the Geyser-phenomena, and sufficient for themselves to account for all "metamorphic" changes, by the incalculable dissolving (chemical) power of waters overheated (under pressure of their own column, or either, of confining rocky strata)—and cause them to force apart the fissured heated rock, down to the very lava not itself necessarily acting under any explosive stress; by which contact—as by a fused metallic mass exploding water-pipes—the explosive shock, mainly propagated by the artesian sheets, as an earthquake, it is already presumed by some former authors, can arise under the agitating influence of the spontaneous impact of Plutonic masses, gravitant towards the moon.

It is by the evident oscillatory and reverberative bis-diurnal rising and falling of coasts and continents—vide South America—that, by clashing and recoiling, not only progressive wave-shocks are retarded, as per map; but that by their meeting and redoubled roll: back upon an already sinking border, those heavy tides eventuate both on the eastern borders of the Atlantic, from the Maelstrom clear down—outside of the European inland-seas and ensconcements—to the Cape of Good Hope; and on the eastern border of the Pacific, from Alaska to Patagonia: and the littoral shock that raised the coast so as to ascend, as an "ebb," causes the same wave which returns from the high-seas to recoil upon a border already dragged downward by the heavy plunging (cold) eastern submarine "plateau renversé" of sea-bottom.

The very shock experienced at the coast is a proof of the practical, partial unpliancy of the submarine crust, by showing a deflection of ascending thrust towards the sides! It hence follows that the "planetary elongation" (with its utmost claims to about six feet of difference) has been somewhat checked, having resulted in a local derangement or divergence between land and sea, as ebb and tide, and that is a never-to-be-forgotten source of unequal horizon and unequal "transits" or "star-times" thence infallibly resulting!

Now as to the sea-water, with an impact at six feet differential velocity perpendicularly towards the moon, etc., in advance of centre: it cannot rise much in any place directly under the luminary; it simply diminishes its weight, it is true, and only by collateral influx of masses at an octant (where the impact favors a tangential rush, and that need not leave the crust, underneath, in following their impulse) it is somewhat raised, though little; whereas the actual, although reduced, circulatory gyration, required, of at best six feet diameter, cannot be executed by an entire sea underlaid by a rigid crust; and therefore the motion that, in the Plutonic mass, pivots—and is mutually poised—on the centre, in the case of surface sheets above a crust, is sidewise deflected towards the poles as the elongation of that centre; and it is probably an entire sea-depth of several miles, progressively impinging at a circulatory motion on an almost unyielding platform, that the equatorial east-west current, and actual surface-rush, results as an excuse and partial attempt at an uninterrupted circular current sweeping the equator (such as it no doubt has once existed), and which, collecting all (hot) surface waters, now sends them flirting sidewise, towards the pole as an excuse for a centre, thus producing that returning west-east sense of drift that carries the cold polar undercurrents' exchange-waters south-*east* from the Arctic region—instead of south-west, as tend the trade winds!—and north-east from the South Pole to Peru!

Another consequence of the inequality of sea-grounds is manifest in the evident relation of the Asiatic pelagic chain of volcanic action parallel to its warm gulf-current. It is there that the crust, by its high temperature and consequent tenuity, is most liable to "breaks" or volcanic explosions under the contracting power of submarine cold in the ancient Pacific, aided by the attractive agency of the moon, most powerful in its perigees and conjunctions with the sun, as argued by Professor Perrey, and so disastrously confirmed by the late convulsions at Hawaii, occurring mainly the day preceding the perigee April 3, 1868, and in South America during the weeks of the perigee Aug. 17, 1868, commencing after the date of our adjournment from Chicago (August 12), where the presaging assertion was uttered, before the section, that "the coast of South America, the hottest and narrowest strip between the most stupendous mountain-elevation skyward, and the most precipitous profundal trough-lines or cold-water crests abysmally reflected, has never been inhabitable (witness the unobserved entrance of the conquistadores under Pizarro into Peru), and will never come to rest, being riven and convulsed under the moon, as long as its Andes lie athward the equator;" unless all the most important geographical features of the Globe be changed, and whereof I will here offer a short synopsis.

In the first place, aside of the ancient Altai or Cobi plateau in Asia, we find a north-east range of uninterrupted main-line of mountain-elevation, commencing at the terminus of the Himalayas, at the Brahmapootra river: the continuous rear-range of Assam, China, Mantchooria and Siberia, up to Behring Strait.

This range then is parallel and "twin" (so to say) with the corresponding east Asiatic "volcanic fissure" off-shore, cleaving the main from the explosive Sunda Archipelago up to near Behring Strait, as above detailed.

The Japanese gulf-current is parallel to it, parallel to the whole explosive libration of that coast itself engendered by warm currents near the old Asiatic restricted plateau of the Cobi, once the fabulous "Paradise Lost" of the Chinese!

Is it not probable that the advent of uncommonly cold waters or an uncommon perigee of the moon caused such disruptions along the lines of least resistance? The same applies to the Alleghany and Scandinavian mountains. Opposite Asia, along the same ocean on its eastern borders, in America we find a direct, uninterrupted chain of stupendous mountains and volcanoes directly along the "cold, deep water" edge, and from Alaska to Patagonia. Alongside of its Andes, the ground of all South America is so much depressed, so little elevated above the sea, that in the wet season an inland continuity of water is established between the low river-beds of the Orinoco, Amazon and La Plata, draining an extent as vast as that from our northern Arctic border to the Gulf of Mexico.

A very slight "dip" would cause all South America to disappear from the face of the Globe, leaving the chain of the Andes to play a part like that of the submarine Asiatic volcanic (N. E.) crest.

It is from Behring Sea and the Aleutian Archipelago that the North furnishes the submarine exchange waters which are deepening the Pacific by a cold undercurrent all along the shores of North America from Alaska to Quito.

This abrupt deep channel is overrun with the circulatory Japanese gulf-current, deflected eastward by the interposition of the submarine volcanic Aleutian chain and the cold northerly return-waters drifting in between it and the shores.

THE DEEPENING OF OCEANS

withdraws the waters from the surface, sinking their coral-reefs so often quoted in proof of the ancient shallowness of the open oceans. The same sinking of sea-bottoms also buoys up or denudes, relatively and positively, all continents, and inland seas such as the Caspian, Baltic, Mediterranean, and Lakes; the coast of Norway and Sahara deserts, no less than the saline pampas of La Plata; and in all continents we find huge areas rather suddenly uplifted towards the snow-line; phenomena gradually arriving together with the submergence of the ocean's archipelagoes, and demonstrative of polar undercurrents having been formed probably by the deflection of the original equatorial ring-current.

If the Aleutian chain did not oppose the Japanese gulf-current, then as it might be, the Arctic Sea would fully receive, two rapid, heated currents, conjointly to be drained by Baffin Strait, their only vent left.

The Arctic Sea would then become a warm or temperate one, and by the ensuing dissolution of heavy bottoms, the open oceans would become warmer, and hence shallower.

The islands would become higher, larger, and rather uniformly temperate, and the continents would relatively sink down towards the Plutonic mass, become smaller, lower, and from underneath be on an average more uniformly heated. Also the seas would be less heated above, less frigid below, than at present!

If once the Aleutian Archipelago did arise and thus first deflect the Japanese Gulf, as an ancient hot-water current partly tributary to our Arctic Basin, then perennial frosts must have ensued at the North Pole, and the whole Arctic Sea been sunk, together with all open oceans, by acquiring a denser, colder, heavier under-crust, denuding northerly all the coasts of Siberia and Hudsonia, as it is now revealing that frozen, once submarine "drift of pachydermal carrion," then by a rapidly coursing gulf-circulation conveyed intact to northern coasts, where at Kotzebue Sound, above Behring Strait, the "Herald Expedition" (in search of Sir John Franklin) discovered a denuded, once submarine, ice-plateau of from thirty to six hundred feet height above the modern sea, and several feet in thickness, overlaid by a drift of pachydermal bones and mould of animal origin not yet fully decomposed, as it were; the perpendicular cliffs still jutting down under water to the bottom of a sea on account of its saline ingredients remaining liquid, while too cold to dissolve ice once through freezing separated from the brine.

A slight depression of oceans — as by the lowering of the general sea-bottoms often referred to — would cut off all other shallower gulf-currents, such as the probable ancient Baltic and Mississipean ones, and by also laying dry an Arctic exit (now Mackenzie river and Undine Region) first grate down the slue with drift-ice, and by a final preclusion add to the Arctic rigor thus suddenly supervening.

If at one period the Aleutian chain or the narrows of Behring Strait so far projected over the sea as to preclude the entrance of Pacific waters at that point, it would not only have left the Pacific to circulate and recoil back upon itself — a shallow heated sea deprived of an Arctic undercurrent — but then the excessive cold, produced by a sluggish Arctic circulation only under stress of the Atlantic gulf-current, and then alone issuing back through Baffin Bay with small force; would not only have stemmed and deflected that current at its very entrance, so as to make it recoil upon its own bosom, thus causing a profound, but heated ocean, violently circulating, in its temperate latitudes, with but a scanty undercurrent, in the Atlantic likewise; but it might finally *freeze up Baffin Strait*, and cause the northern basin to stand as an *immovable pack of ice* overrunning all northern border lands with an accumulation of glacial masses, enough to cause, on a smaller scale, that eminently historical refrigeration of all northern borders, which avowedly caused the post-christian *migration of nations* both in the old (Mongols and Swedes) and in the new (Caribs and North Mexicans) world; but the *rising of the Iceland-shoal* would also fully account for all the features of a "glacial epoch" which, as Professor Tyndall so aptly suggests, requires as much a hot climate on the one, as a congelative, immovable, cold one on the other side!

And it is this explanation I here offer, of a temperate universal middle zone of our Globe, adjacent to a pitiless cold north pole, and productive of the incredible extension of glacial formation in the first place, by a *non-deflected circular equatorial current* (somewhat like the ring of Saturn); secondly, on a smaller scale, by a partial preclusion of Pacific and Atlantic influx—through the Aleutian Islands and the Iceland-shoal—on the one hand.

And as to the disappearance of that period, and wasting of its glaciers, I offer this explanation, that the *breaking-up of the equatorial current, and the opening of Behring Strait*, or the submersion of a more projecting Aleutian barrier, caused the seas to sink by a cold exchange undercurrent, and the glacial accumulations to melt away by the advent of warm currents; and if that barrier continues to sink, the Asiatic current might yet more fully enter by Behring Strait, and leaving by Baffin Strait, in quick exchange still cause all the seas to rechafe at bottom and rise, and our continental expanses to once more sink toward the Plutonic fires; their Neptunic strata, their swamps and peat-bogs once more to transmit to following generations the phenomena of "metamorphic" rock, of "stone-coal" for peat, and the petrified impressions of the world we live in at this present period!

In conclusion let me remark, that what on broad evidence we can at large construe, on granted principles and given causes, is far more reliable than what we have to explain by hypothesis alone, and on often quite irrelevant "exact measurements" where the very minuteness or sensuous "exactitude" required, is often the root of latent error; whereas, when by *logical* exactitude in our synthetic applications of the known; by a liberal exegetic

GENETIC UNDERSTANDING:

of causes and effects; of logical premises and inferences; and (in the organic world) by the study of developments in their commencement and end: when by an intelligent appreciation of the normal we can substantiate the distinctive or "abnormal," and science on its evidence instead of on *credence:* then, by understanding the motor agencies of the present, we may explain the past and control the future; above all, it will point the way whither to direct our suggestively intuitive energies, our labors and observations, both plausibly and profitably.

EQUILIBRIUM AS EQUAL VELOCITIES.

All the foregoing phenomena, as well as all barometric ones, and both the atmospheric and hydrothermic exchanges from the Pole to the Equator, and *vice versa*, all being based on the condition of a *horizon of equilibrium* I would here briefly submit a few considerations.

If a solid globe of homogeneous attractive power is set to rotate, then the resultant of forces will cause the pendulum as well as any other mass to "vary," both as to velocity and the direction of each resultant: which resultant effect withal constitutes "weight" proper; namely, the compound effect of the theoretic attraction of masses and of the sublevatory effect ("flirt") of tangential inertia, which sublevatory, antagonistic resistance to the "attractive" factor, I would here beg leave to plainly designate as "rotatorial dip."

If under such assumption of "true sphericity" the pendulum must vary (for a planet, of the bulk and rotation as those of ours, computed at the proportion of about $\frac{1}{289}$), if *not* aplanated: how then should it be consistent to state, that (the observed) "variation" (of pendulum) should argue aplanation? If really so observed, would it not argue sphericity? This is the question.

If, however, we consider a planet adaptable according to hydrostatic necessities, not only as by an external liquidity (as of oceans) but also by a probable profundal liquidity of Plutonic, fused or pliant, hydrostatically "terrigneous" masses; if, as we actually find it, the presence of the ocean level both at the poles and the equator, and also the uniform barometric height at all sea-borders argue a true horizon of equilibrium, how is it that the pendulum should be said to vary, and that "variation" to be *corroborative* of aplanation; whereas, aplanation, such as geodesically measured, argues a form of *equal* hydrostatic surface velocities at right angles to tangents, —or, in the sense of the "normals"—thereby producing a uniform, connected, hydrostatic surface, and which otherwise could not obtain! Why or how is it, that the argued *compensatory elongation of the Globe* should not work as a "compensation on surface particles?"

If the variation ought to obtain in case of sphericity, then it ought not to obtain in cases of equilibristic aplanation. If such variation proves sphericity, the same one (approximately so) cannot prove aplanation.

In all popular and established school-books, and Astronomies, I find at certain junctures the factor of "attraction" substituted by "gravity," which grammatically means and is weight; technically, however, is meant for only one of the distinctive factors of weights whatever; namely, Newtonian mass-"attraction." This is a dangerous usage of an ambiguous expression, for immediately follows, *e.g.*, in Herschel's "Outlines of Astronomy" (¶ 231) a commutation or substitution like this, "there is actually observed to exist a difference in the gravity or downward tendency of one and the same body," etc. Here "gravity" is urged in its grammatical sense, instead of the technical distinction, and at that in order to initiate the (true or apparent?) contradiction of a compensatory or "equilibristic" form *not* producing compensation or "equilibrium!" This seems not to be consistent. Or, can it be said to be consistent?

It ought to be recollected, that when the question is of hydrostatic pressure, the weight cannot come into consideration neither as "specific" weight, nor as "absolute" weight, for neither are solid weights meant nor are hydrostatic weights absolute, but (like any other weight whatever) entirely dependent on the extraneous masses! Nor are these weights detached masses! We speak of a "weight" of one ton or two tons—this comes not into consideration; nor either "specific" weight. All that can come into consideration in hydrostatic phenomena, is the resultant *velocity* and that in a compound sense of *direction* resulting from two distinct factors, and which resultant or perpendicular surface velocity in a case of elliptic aplanation is not mutually reflected only by the absolute height of the column (as, *e.g.*, in the case of close proximity to a uniform level or horizon); but each column has its own index of velocity; namely, in the short or polar axis pure attractive velocity, and in the sub-equatorial dimensions both the "attractive" velocity and its sense of direction modified and diminished by the rotatorial dip velocity; and if, as it is agreed, and must be, the "force" of each channel is equal—as required for equilibrium or "equal impinging velocities"—it follows that as a higher equatorial column, when diminished by flirt still exercises equal power with the (inscribed) polar radius; it must be that each measure of the longer column is operated upon by a lesser compound power than the same geocentric measure of the polar column is operated upon; but that by the urging of the polar column down to the centre, so much "head" has been bulged up into the equatorial column as to produce an increased amount of attraction in that sense; that, short of $\frac{1}{590}$ relates *conversely to radii*, and that, although diminished by the incident tangential loss by the dip or flirt, yet leaves a resultant velocity toward the surface (not one of masses but of a hydrostatic thread), equal to the velocitarian impact at the apex of polar column. From a mere arithmetical point of view, is it not plain that although each particle or measure has been rendered lighter in the equatorial sense, still the *height* of that column or its "corresponding points" (fractions) are raised to such an extent as to *attract more powerfully as a whole?* for being able to just balance the loss arriving through flirt? so that, *e.g.*, supposing the liquid equatorial column to be thus far heightened as to draw, by mere attraction, any falling body seventeen feet per second, and that second's dip amounting to one foot, this would still leave the compound resultant velocity sixteen feet per second towards the surface!

Nor can a *surface be maintained* except under the identical impact at velocities collaterally exercised each in its own resultant, and producing rectangularly a complete "surface" of virtually "equal stress," but of different distances from the centre.

Therefore, in a "figure of equilibrium," the rectangularly impinging surface-velocities in order to agree with equilibrium must be *equal;* and hence, in a true figure of equilibrium, the *pendulum*, the lead, a stone flung, ought to "bear" equally all over the surface!

Still, after having once fully "compensated" between the factors of pure attraction and that of rotatorial fling, La Place, Herschel, and others, directly proceed to deduct the flirt *once more* from the uniform compound result (namely, equal stress or "weight): and this only in order to usher in the received "pendular variation."

How is it then that in a form of compensatory velocities, where the spherical form is denied, the compensation should not appear achieved in the pendulum, likewise?

I think the question opens. What the *pendular data*, by experiment, actually were? I have no records to find out. All I can find are tables of "reduced" pendular results, by which, if conducted after the form which Arago in his Popular Astronomy, book 23, chapter 12 (but perhaps too loosely only) gives: by referring positive results, originally equal enough, instead of "to the sea-level" (as required and also averred) actually to *an absolute distance from centre* the actual collateral equality would have been forcibly retranslated into the diversified rates of radial planetary compensation, and a true compound equal result been made to show only differences of *radii* supplied, but not of strokes, and but just themselves required to produce that actual "equality" of pendular velocity!

I here recommend the whole idea, as popularly received, together with the (true or apparent) incongruities involved, to the renewed attention of philosophers, in order at least to find a form of linguistic communication to us "outsiders" to remove such (true or apparent) inconsistencies, which, necessarily, only contribute to confuse and deter the student of geophysical phenomena.

B. NATURAL HISTORY.

1. On the Evidences of the Antiquity of Man in the United States. By Colonel Charles Whittlesey, of Cleveland, Ohio.

Within the past five years remarkable developments have taken place in Europe connected with the antiquity of man; not less then a dozen books have been published upon the recent discoveries, most of which are written by men of learning, and show the results of their personal observations. The number of the relics of man thus brought to our notice is surprising. In the delta where the impetuous Tiniere discharges into the Lake of Geneva at Villeneuve, in Switzerland, Mr. Morlot has traced the successive occupants of that country from the Christian era back to the age of Stone.

The savans of Switzerland have discovered and explored large numbers of the habitations of a departed race, who built their domicils upon piles, in the shallow water of the lake near the shore, and who were also of the era of bronze, and stone implements. Retiring still farther from the historical epoch, the French and Belgian geologists and ethnologists have found, in the numerous bone caverns of those countries, utensils fabricated by men, and bones of the human race, under circumstances which have led these investigators to date their existence nearly or quite to the glacial period. These announcements by the continental explorers were received by the English geologists with astonishment, and in most cases with incredulity.

In 1863 the patience of the English investigators was nearly exhausted, by reports made by M. Boucher de Perthes, at Abbeville, on the banks of the Somme, that he had exhumed a human jaw from the superficial materials of that region. He had in 1837 and 1844 announced the discovery in the same beds at a depth of ten to twelve feet, of numerous wrought implements in flint, made from the silicious nodules of the chalk formation on which the Abbeville drift materials rest. During a quarter of a century these reports were generally discredited. M. Boucher de Perthes was not a man of science, but a gentleman of wealth and leisure, of high literary acquirements, with much French enthusiasm, and he had a theory to support. His brilliant general learning rather injured than improved his authority among those who pursue the exact sciences. He adhered to the doctrine that the flood of Noah extended over the earth, and that the knives and arrowheads in the drift of the Somme were the remains of generations which were destroyed by it. To find evidence in support of this belief, he pursued his investigations of the gravel beds around Abbeville, until one generation has passed away. His proofs were thus far only sufficient to convince a few personal friends of his own nation. His own life had passed the usual limit of human existence, yet the ardor of youth was in his case coupled with the perseverance of age.

A number of other fortunate circumstances combined to render his researches more thorough than any heretofore carried on in the drift materials. There is a canal in the valley of the Somme, which in one place gave a fresh section sixty feet in height. The railway from Bologna to Paris passes through Abbeville, and up the Somme.

For many years the common roads of Picardy have been made and repaired with the flint gravel of the drift beds, which was procured from numberless pits along the banks of the Somme.

The French government has built a large permanent fort near the residence of M. Boucher de Perthes, at Abbeville, in the ditches of which the same beds are exposed. M. de Perthes had for many years engaged the workmen upon all these improvements to search in the gravel for flint arrowheads, hatchets and splinters, by paying them a small sum for what they should find. He had also the good will of the Emperor, and consequently of the superintendents of the works. He spent much of his own time with the laborers in the pits, and had friends who did the same. His faith in the existence of human bones in the same beds where the flint implements were found, never flagged, because he considered that he had himself seen decayed fragments of them; not, however, perfect enough to announce to the world.

On the 28th of March, 1863, the great object of a long life was accomplished by the discovery in a pit at the wind-mill of Quignon, about seventy-five feet above the tidewater at Abbeville, of one-half of the lower human jaw, near which were some human teeth. During the year which followed, several other fragments of human bones were found in a similar situation, in a layer of gravel near the overlying chalk, at depths of ten to twelve feet. These were in close connection with the flint implements, but the human remains were so small, and with the exception of the teeth so much decayed, that the workmen very seldom noticed them. They had, moreover, a superstitious dread of human bones found in a position so mysterious, and therefore rather avoided, than sought them. Most of these remains were discovered by scientific collectors, who broke open with their hands the lumps of clay and sand of which they formed the nucleus, which the workmen had thrown into the spoil banks. It is evident that nothing short of the presentiment, the faith, and the enthusiasm of M. B. de Perthes, making the most of the most favorable circumstances, would have brought these relics to light. Without all these happy coincidences the discovery might not have been made during another century. His disclosures led to close investigations along the valley of the Somme, and the bluffs which border upon it, up the stream to and beyond Amiens, a distance of about thirty miles, the formations being everywhere the same. Hundreds of flint implements have been exhumed from the same gravel beds, on the French side of the channel, and on the English side, in formations precisely the same; amounting in all to about (3 000) *three thousand.*

As to the denial made by the principal English archæologists regarding the genuineness of the jaw described by M. de Perthes, I refer to the report of Milne-Edwards and others in the "Compte Rendus," from May, 1863, to July, 1864.

If we cannot rely upon statements supported by such an accumulation of proof as is there produced, we must refuse credence to most of the reported facts in relation to all other fossil remains.

AGE OF THE ABBEVILLE GRAVEL BEDS.

It may not be true that the superficial materials in which these remains are found, are of the same age as the North American drift. They are, however, the only stratified beds between the soil and the cretaceous strata in that region, and they occupy a very extensive region in France and England. These beds contain the fossil horse, ox, Elephas primigenius, and rhinoceros, and constitute what is known in Western Europe, by the name of diluvium. It is quite probable that those wide spread diluvial layers belong to a modified condition of the drift materials, corresponding to what we observe here in large valleys near the southerly border of the boulder drift. As the glacial period drew towards a close, the intervention of ice as a transporting agent became less, and that of water greater. The European diluvium probably belongs to the later phase of this era.

Some explorers detect the action of large floes of ice, in the movement of blocks belonging to strata foreign to the region; but most of the materials are of local origin, belonging to the valleys of the present streams. In general, the mammalian remains are the same as in the drift of the United States. Along the great valleys of Western Europe, such as the Seine, the Rhone, and the Rhine, the loess is a conspicuous member, generally the upper one. It is the same in the valley of the Missouri, the Mississippi and the Ohio. The European diluvium is a fresh-water deposit, like that of the western and northwestern drift deposits, on the waters of the lakes and the Mississippi. It embraces, like our drift beds, many varieties of timber and vegetables which are more northern than the present vegetation. The fresh-water and land shells which are embraced in the superficial materials here and in Western Europe, are almost identical. So many resemblances go far to establish a geological parallelism.

Mr. Lyell inclines to the opinion that the flint bearing beds of Amiens and Abbeville are more ancient than the bone layers at Natchez, Mississippi, which are at the bottom of the loess.

Whatever may be the exact order of arrangement, there is a general synchronism between the post-tertiary or quaternary system of Europe and the United States, and since human relics have certainly been found in this formation in the old world, it is reasonable to expect them here. If human bones exist in our northern drift or in the modified river valley deposits of the terrace period, generations may pass before they are discovered. Flint and stone implements are imperishable, but the osseous parts of animals and men are dissolved by chemical action. In beds of such great antiquity, historically considered, as the lower quaternary, very little is preserved except the teeth. Such discoveries must, therefore, be due in this country, principally to accident, and not to research.

We cannot expect to have the concurrent aid of an enthusiast like M. Boucher de Perthes, a government ready to assist, and thousands of workmen turning over the earth, in which such revelations may be expected.

ANTIQUITY OF THE NORTH AMERICAN RED MAN.

It is a little more than three hundred years since the Spaniards landed on the shores of the Gulf of Mexico, where they found the red man, in every respect the same as he is now, among the southern tribes of the United States. If we can credit the historical literature of Iceland, the Northmen were on the coast of New England about the year 1000, where they met a savage people, apparently the same as our forefathers encountered there six hundred years later. The same tribes of Indians, remnants of whom have survived to our day, were seen during the previous one hundred years on the St. Lawrence and the Atlantic coasts, by Jacques Cartier, Americus Vespucius, Verrezani and the Cabots.

By historical proofs that are now regarded as worthy of credit, we can thus trace the North American Indian, as a resident in the territory of the United States, backward more than eight centuries and a half. In this as in all countries there are found the remains of a people more ancient than any written record.

The North American Indian has always occupied a position so low, in comparison with other races, that permanent monuments of his presence are very rare. If he had disappeared before the advent of an historical race, no certain traces of his existence would have been found. His tenements were temporary shelters, which he carried from place to place. The mounds he raised over the dead were low, and contained relics of which the most that can be said is they belonged to a people in the rudest state of barbarism. Where he cultivated the soil, it was in a manner so slight that a few centuries of time obliterated the evidences of it. The scattered inscriptions and effigies which he cut upon trees and stones had no meaning to any one but himself. His tools and implements had no special characteristics to distinguish them from those of any other savage people in the lowest scale of development. Since the historical period in North America he has remained precisely the same, without progress towards civilization, and therefore it may be inferred that his condition in previous centuries while an inhabitant of this country was a fixed one. He could not have descended much lower, and he has left no evidences of an advance. The effect of this fixedness of condition is to render the deductions of craniologists of more value than they would be in the case of a people whose mode of living, whose thoughts, occupations, dress, religion, intelligence, food, and general surroundings were subject to change, especially in the direction of progress.

How long have they been in the occupation of North America, and how can we determine the period?

The antiquarians of Europe regard those ancient people who used *flint implements*, as having been prior to those who had implements of *stone*, and the latter as being older than the races who had the use of bronze, or other metals, especially iron. In the United States the race next prior to the white one, had very few implements of stone. Their knives and arrowheads, their war implements, and their agricultural tools, were almost entirely of flint.

They had a very few and very rude cutting instruments of native copper; possessing at the same time in a certain degree, implements of the flint, the stone and the metal era of Europe, the flint greatly predominating. The mound builders, who preceded the race of red men, produced and used tools in the reverse order. Their *stone* axes, adzes and mauls, were very numerous, *copper* tools plenty, and those of *flint* very rare.

In their case the most ancient people were the most industrious, and cultivated the soil, possessed more mechanical ingenuity, and left more prominent monuments, which in the course of nature will never perish.

The difference between the relics of the mound builders and the Indians is so great that there is little difficulty in deciding between them.

On the Atlantic coast, from Nova Scotia to Florida, there are numerous refuse heaps of sea-shells, which are almost identical with the ancient shellheaps of Sweden, Norway and Denmark, known in those countries as "Kjœkkenmœddings."

Those of the Eastern and Southern States, have been examined by Mr. Jones, of Halifax, Professor Wyman and others, who regard them as the work of the North American Indians. On one of the shell mounds of Florida, situated on the river St. Johns, there grew an oak, which was five hundred and fifty years old. There are very ancient shell mounds on the Tennessee River, between Chattenooga and the "Muscle Shoals," but they have not been thoroughly explored. I have also seen them on the Wabash in Indiana, near New Harmony. The American shellheaps, like those of Scandinavia, contain stone and flint implements mingled with charcoal, and the bones of animals, fishes, birds and reptiles, now or recently living in the vicinity. Only one small bone of man has been found in the American kitchen-muddins.* They have neither the size or the marks of antiquity, which the Danish shellheaps exhibit but are some of them so ancient that changes of climate and of vegetation have occurred while they were being formed. The shellheaps, therefore, as at present known, give us very little information in regard to the antiquity of the red man.

But there are remains of the same race in bone caves, and rock shelters, which bear directly upon this question, to which I will call your attention.

* Since this paper was read, a human *underjaw*, with teeth, has been found by Prof. S. F. Baird, at the Shellheap on Eagle Hill, Ipswich, Mass. For an account of this and other Shellheaps in New England, see Prof. Wyman's articles in the "American Naturalist," Vol. I. Later investigations than those reported by Prof. Wyman have shown the deposit at Eagle Hill to be much larger and deeper than was at first supposed.—EDITOR.

ELYRIA SHELTER CAVE.

Examined in April, 1851, *by Dr. E. W. Hubbard, Professor J. Brainerd, and Charles Whittlesey.*

This is one of numerous instances where the "grindstone grit" of northern Ohio, resting upon soft shale, presents a projecting edge, forming a grotto capable of sheltering a large number of persons, being about fifty feet in length, by fifteen feet broad. This and others in the vicinity which have not been explored, correspond to the European "shelter cavern," where human remains are always found. These retreats constituted the domicils of our race, while in their rudest condition. We dug to a depth of *four feet* on the floor of this cave, composed of charcoal, ashes, and bones of the wolf, bear, deer, rabbit, squirrels, fishes, snakes and birds; all of which existed in this region when it became known to the whites. The place was thoroughly protected against rains. At the bottom, lying extended upon clean yellow sand, their heads to the rear and feet outwards, were parts of *three* human skeletons; two of them nearly entire. Two of the skulls were preserved by Prof. Brainerd. They were decided to belong to the North American race of red men, by those who had an opportunity to examine them. These skulls were exhibited at the Cincinnati Meeting of the American Association in 1851, but were afterwards destroyed by a mob, together with the entire Museum of the Homœopathic College, at Cleveland. The position of the skeletons indicated that they were crushed by a large slab of the overhanging sandstone falling upon the party, while they were asleep at the back part of the grotto. One of the skulls was that of an old woman, the other of a young man. Flint arrowheads such as the Indians once used, were scattered throughout this mass of animal remains.

This cave may be found on the west bank of Black River, a short distance below the forks, at the town of Elyria, Ohio, in a romantic gorge, through which the river flows. Judging from the appearance of the bones, and the depth of the accumulations over them, two thousand years may have elapsed since the human skeletons were laid on the floor of this cave.

HUMAN REMAINS IN A CAVE NEAR LOUISVILLE, KENTUCKY.

T. R. Scowden, C. E., 1853.

In constructing the reservoir for the Louisville water-works, on the bluffs of the Ohio, two miles above the city, the Engineer, T. R. Scowden, Esq., discovered a cave in which were a large number of human bones. It is forty feet from a mural face of lime-rock of the Upper Silurian epoch, which is known in Kentucky as the "cavernous limestone." The elevation of the bluff is about one hundred and twenty-six feet above low water in the river, and ninety feet above the bottom lands, which are half a mile wide in front of the water-works. It is probable the cave is an extensive one. No outlet is known, and when water was directed into it, no place of discharge was discovered. As far as it was explored the opening was not large. It had a direction downward and to the rear, but was so much infested with rattlesnakes that no one could be induced to examine it. On the rock there was ten feet of the loess like loam of the country, in which was a depression, into which the surface water settled, such as in that region are called sinks. The bones, a box of which were preserved by Mr. Scowden, were cemented into a breccia, by calcareous drippings from above. In one mass there are portions of six human crania, but none of them large enough to be of value in the comparison of races. There are other bones and teeth, representing more than that number of persons, which are in a good state of preservation. The opening in the rocks at the top of the cave, which was closed by a loamy clay, was not as large as the cavern, the roof of which was twelve feet below the surface of the lime-rock. From the roof there were the usual pendant concretions, known as stalactites. In shape this part of the cave was a dome, six feet across at the base, and about five feet high, the bones lying in a confused heap on the floor. The downward passage into which the water flowed was situated at the rear, and its direction was away from the bluff. A stone axe and a pestle were found with the bones; also a flint arrowhead. Below the cliff there was an ancient Indian burying ground, in which many graves and human bones were exposed while digging the trench for the main inlet pipe of the water-works.

The bodies may have been introduced for burial, through a distant entrance not yet discovered, or there may have been a time when the cave was open above. They were evidently of the Indian race, and the place was a sepulchre. Among the Hurons who lived between Lake Ontario and Lake Huron, when the French missionaries were there, two centuries since, there was a practice of collecting, from time to time, the bones of their dead from all the graves of the tribe. They were then placed in a pit, without order, and covered in the presence of all the people, consecrated with funeral ceremonies and lamentations. The cavity or sink in the earth, at Louisville, would constitute a burial pit already made, or partially made; and after the bones were deposited, they could have been easily covered. From the quantity of tufa formed on the roof and over the bones on the floor, it is evident that a long period has elapsed since they were deposited—full as long as in the case of the Elyria grotto, or say two thousand years.

HIGH ROCK SPRING, SARATOGA, NEW YORK.

Profile by Dr. Henry McGuire, 1866.

1. Muck and tufa on which the cone rested . .	7 feet.
2. Tufa	2 "
3. Vegetable muck, on the surface of which lay a pine tree, with one hundred and thirty annual layers of growth, worn by the feet of persons, probably Indians, ...	not given.
4. Calcareous tufa, same as No. 2	3 feet.

Chancellor Walworth states, that in 1825 he saw at St. Regis, New York, an old Mohawk, by the name of Loren Tarbel, who said the water did not flow over the cone when he was a boy.

The estimates made by Dr. McGuire, of the time occupied in the formation of the tufa, which showed twenty-five layers to the inch, is, for five feet, fifteen hundred years.

Rate of formation of the rock cone, eighty years to the inch, is, for four feet, equal three thousand eight hundred and forty years (probably too large). For the accumulation of muck, five hundred and ten years (which is probably too small). Total, five thousand eight hundred and forty years. The pine tree worn smooth by the feet of men, beneath the upper bed of tufa, must have been there at least one thousand years before the formation of the rock cone.*

Mr. Koch, who furnished a skeleton of the *Mastodon Ohioensis*, from the recent alluvium of the Pomme de terre River, Mo., to the British Museum at London, convinced the English geologists, that he found a flint arrowhead at the depth of fifteen feet beneath the skeleton, which arrowhead was of the pattern used by the North American Indians. He also stated that near the skeleton, and full as deep, were three other flint arrowheads. If these statements are reliable, they tend to extend the antiquity of the occupation of red men much beyond that of the American shellheaps, in which are no remains of extinct amimals. This statement of Mr. Koch, is, however, contradicted by one of the men who assisted him in exhuming the skeleton. A similar case is presented by Dr. Holmes of Charleston, S. C., in the "Proceedings of the Philadelphia Academy of Natural Sciences," for July, 1859. He found pottery at the base of a peat bog, on the banks of the Ashley River, in close connection with the grinder of a mastodon.

This pottery probably belonged to the red man, and if so, strengthens the proof of his presence here before the horse, mammoth and mastodon were extinguished.

A PEOPLE BETWEEN THE INDIANS AND THE MOUND BUILDERS.

The ancient earthworks which abound on the waters of the Ohio do not extend northward to Lake Erie. There is a belt of country north of Central Ohio, near the water shed of the streams that empty into the Lake, which is without ancient works, so far as at present known. On all the rivers discharging into Lakes Erie and Ontario from the South, there are ancient forts in profusion, but they are of a type entirely different from those in the valley of the Ohio, which extend southward through Kentucky, Tennessee and Texas, into Mexico.

*Dr. McGuire has republished an account of this excavation at High Rock Spring, in the "Proceedings of the Boston Society of Natural History," Vol. xii, p. 398 (May, 1869), more in detail than the above, but giving nearly the same total result (5470 years, within all bounds). He also mentions the finding of a fire-place and charcoal on the clay bed, under a stratum of muck 2 feet thick, underlying the last 3 feet of tufa mentioned above.—EDITOR.

There are few mounds and no pyramids or rectangles on the shores of these lakes, but hundreds of small irregular fortifications of earth. They are always located in places which have natural strength, such as bluffs and points of land with water near by, and steep ravines on two or more sides. Generally the fort consists of a ditch or ditches, and embankments across the narrow part of a peninsula. They are almost without exception well selected strongholds, judiciously fortified, and have the appearance of long occupation. The people who occupied these forts doubtless comprised not merely soldiers, but the entire population of the country. They must have been cultivators of the soil, but divided into hostile clans, like the old Scots and Germans, who required castles as a defence against each other. This race of fort builders on the lakes may have been contemporaries of the mound builders, and of the effigy builders of Wisconsin; but they were of prior date to the Indian, who has no more knowledge of the origin of these forts than of the mounds, and who had no earthworks when the whites first encountered them. Mr. Squier in his "Antiquities of Western New York," attributes them to the Indians, but upon grounds that do not seem to me sufficient.

I introduce this intermediate, or if not intermediate, distinct race, as the basis of future investigation; but do not feel warranted at present, in using it to increase the antiquity of the American man. I confine myself to the red men, the mound builders, and to the evidence of fossil men, contemporary with the elephant, mastodon, horse megalonyx, and other mammalia of the quaternary.

THE RACE OF THE MOUNDS.

The difference in their modes of burial, indicates clearly that the mound builders were a people distinct from the North American Indians. In almost every ancient burial mound, the remains of both races have been interred; the bodies of the most ancient at the bottom, or at great depths, on charcoal hearths, in rude enclosures of wood or stone, with copper ornaments, implements, wrought shells, coarse cloths, and other peculiar marks. On the sides of the same mounds, some of which are seventy feet in height, are the skeletons of the red race, at shallow depths, with no attempt at stone or wooden coffins, and in the early graves no metal ornaments. In the sepulchres of the mound builders, flint knives are rare, and flint arrowheads still more so; but stone axes are not uncommon. In the Indian graves are numerous flint arrowheads and knives, but stone implements, except pipes, are very rare. The Indian has nowhere raised conspicuous earth mounds over his dead. Their most notable monuments, are low stone-heaps, like the Irish cairns, formed gradually by the friends and admirers of the deceased, as they pass the spot, throwing a small boulder upon it. In the Indian graves are no evidences that the body was burned, as there are in large numbers of the old earthen *tumuli*. No instance is reported where an earth mound has beneath it the remains of a race which might be more ancient, but the instances are numerous where there are bones of the mound builders beneath those of the red man.

The mound builders show in all their relics and their work, a greater mechanical skill, more intelligence, industry, and perseverance, than the Indian tribes ever possessed. They wrought the copper mines of Lake Superior very extensively for the purpose of fabricating tools of this metal, which are found in their mounds, in contact with their skeletons. The Indians had only the rudest copper implements, and these were of a pattern quite different from those discovered in the mounds. It was a very rare circumstance that the French missionaries found among them copper that had been wrought. It was generally treasured up as a manitou, in the original form of a nugget, and transmitted as an heirloom from generation to generation. The copper adzes, axes, chisels, spades and spearheads, of the mound builders, were nowhere found in use among the living tribes, nor any tools having the same degree of finish, or intended for purposes of so high civilization. They had only the rudest knives, arrowpoints, and implements for curing skins.

These facts sufficiently attest the presence of different races, and at a different period; the builders of temples and burial mounds being the predecessors of those who constructed no monuments or fortifications of earth. We must also infer that a people sufficiently numerous to work the mines of Lake Superior throughout the copper region, and to construct works such as those at Newark, Marietta, Circleville, Portsmouth and Cincinnati, must have been permanent occupants; but how shall we determine the length of this period of occupation.

After examining the principal works of the mound period, in Ohio, and their mine works on Lake Superior, I could not estimate the period of their occupation at less than one thousand years, with a strong probability in favor of two thousand. If we drop for the present the supposition of an intermediate race, and take the highest estimates for the occupation of the red man and the mound builders, we have only four thousand years, which does not carry us back to the beginning of the historical period, in Asia and Africa.

In Switzerland, relics of man in the recent alluvium attest his presence there, according to M. Morlot, nine thousand to eleven thousand years, or beyond the historical records of the old world several thousand years. It is highly probable, and is in accordance with the analogies of Europe, that since the glacial period, there was a people here, more ancient than the mound builders, but I know of no remains of such a people, except the charcoal beds at Portsmouth, Ohio, in the ancient valley alluvium. The whelk, found by Mr. Cleveland, is a shell which was in common use by the mound builders. The alluvium in which it was embedded, is of the most recent kind, which at the mouths of the tributaries of the Ohio, forms very rapidly. At the mouth of the Great Miami, the mud deposited from back water in a single flood of the Ohio River is sometimes several inches in thickness. Since the settlement there in 1789, the stumps of trees and large logs have been covered up in this way, until the plough now passes over them. This modern alluvium is easily distinguishable from the ancient river alluvium, which is derived from the drift materials, and which must also be distinguished from deposits of the terrace period, known in Ohio as the valley or modified drift. The terrace period represents the closing phase of the glacial era, when the valleys were full of water and floating ice, sorting the materials on a broad scale, in lines parallel with the streams. The ancient river alluvium is due wholly to the currents of streams acting upon the valley drift of the terrace period, at levels higher than the present channels.

In both these deposits the elephant and mastodon are common, as well as in the recent alluvium. In the ancient river alluvium has been found the gigantic beaver, or *Castoroides Ohioensis* of Nashport, Ohio, the taperoid jaw of Yellow Creek, Columbiana County, Ohio, and probably the *Bos bombifrons* of Trumbull County, Ohio.

There is, therefore, a long period of time to be accounted for between the earliest mound builders in this country, and the earliest inhabitants of the Swiss Lakes and of the Nile.

Since the close of the terrace period there has been no material change in the surface of Ohio, and perhaps no general change in the climate. Man could have resided here then as well as now. In Belgium and France relics of man extend much farther back, into and even beyond the terrace period, when the surface of the country was somewhat different from what it is now. He lived, and may have perished, with the elephant, mastodon, rhinoceros, and the extinct elk, during the closing portions of the ice period. It is therefore reasonable to suspect that man existed in North America with the extinct elephant, mastodon, megalonyx, horse, beaver, and the peccary of the United States, which lived towards the close of the ice era, though it does not follow that they, and he, were exactly cotemporary here with the European species.

With the exercise of the same never tiring research displayed by M. de Perthes, and the same facilities, it is highly probable that the drift clays of Lake Erie, and the valley of the St. Lawrence, would furnish us with equally palpable specimens of the ancient man.

EVIDENCES OF MEN MORE ANCIENT THAN THE MOUND BUILDERS.

In 1838 while examining the structure of the fluviatile deposits on the Ohio River, at Portsmouth, Ohio, I saw in two places in the east part of that town, the remains of very ancient fires.

At low water, and thence up to a height of twelve and fifteen feet, is a bed of sand and transported gravel, containing pebbles of quartz, granite, sandstone and limestone, derived partly from the adjacent carboniferous and Devonian rocks, and partly from the northern drift, the upper part much the coarsest.

On this is a layer of blue quicksand, from one to five feet thick, in which is a timber bed, including large numbers of the trunks, branches, stumps and leaves of trees, such as are now growing on the Ohio, principally birch, black-ash, oak and hickory.

Over the dirt bed is the usual loamy yellow brick-clay of the valley, fifteen to thirty feet in thickness, on which are very extensive works of the mound builders. In and near the bottom of this undisturbed homogeneous river loam, I saw two places where fires had been built on a circular collection of small stones, a part of which were then embedded in the bank.

The stones were colored red by heat, and among them was charcoal, covered by the clay, of which I have specimens. Around and near to the fire beds, were what appeared to be the exterior membrane-like covering of river shells (unios), but no shells. It was several rods from one of the charcoal beds to the other, and they were not precisely on the same level. They were from eighteen to twenty feet above low water, and about fifteen feet beneath the surface. There are no trunks of trees in the loamy brick clay which is not laminated. It was reported that some of the trees in the blue stratum below had been charred. On the surface of the clay deposits near the fire beds were two parallels, portions of ancient earthworks extending to the river. At the west end of the town—where an artificial mouth of the Scioto was formed, about thirty-five feet deep, in order to allow the Ohio canal to enter the river—the blue quicksand bed and the loam above it dips westerly down to the level of low water. To the westward of this artificial mouth the recent alluvium of the Scioto overlies the yellow loamy clay, cutting off both the quicksand and the loam. The Scioto alluvium has a darker color, is frequently contorted and laminated, and has embedded leaves and branches of trees now growing on its banks.

Francis Cleveland, Esq., an engineer upon the State works, who, in 1828, had charge of the deep cut, informed me that at a depth of twenty-five feet in this alluvium several conch shells were taken out, which were five to eight inches in length. He said they were the same as one I then exhibited to him, procured from an ancient mound on the Scioto River, and which Professor Kirtland determined to be a *Pyrula perversa* from the Gulf of Mexico and Chesapeake Bay, and called whelks. These shells were in common use by the mound builders, probably in their public ceremonies.

Here we have within the limits of the city of Portsmouth, memorials of the mound period, and, as I conceive, of the rude fires of men of still higher antiquity.

REPUTED EVIDENCES OF MAN IN THE QUATERNARY.

Profile of the Mississippi River Bluffs, at Natchez, Mississippi, by Professor C. S. Forshey, 1842.

1.	At low water mark, slate, thickness unknown, probably cretaceous, . .	5 feet.	
2.	Recent conglomerate, probably quaternary,	8 "	= 13 feet.
3.	Coarse gray water-washed sand, quaternary, (high water mark at 40 feet), . .	115 "	= 128 "
4.	Yellowish brown homogeneous calcareous loam, with fresh-water and land shells, the equivalent of the loess of the Rhine,	50 "	= 178 "

On the surface of the sand, the skeleton of a mastodon was found, which appeared to have been buried erect.

Sir Charles Lyell in 1847 (Antiquity of Man, p. 200), collected at this place twenty species of *Helix*, *Heliciana*, *Pupa Cyclostoma*, *Achatina* and *Succinea*. And in the marly layers *Lymnea*, *Planorbis*, *Paludina*, *Physa* and *Cyclas*.

A narrow gully called the "Mammoth Ravine," near Natchez, originated in the convulsions of the earthquake of 1811, is seven miles long and sixty feet deep.

The *Mastodon Ohioensis* in the bed next below the loess was found in connection with the horse, Bos, Megalonyx, and other mammals.

Among these bones Dr. Dickson found the pelvic bone of a man. It was colored black, like the other bones of this bed, but so are the recent bones of Indian graves in the neighborhood. The superficial materials of the Somme in Picardy may be older than the Natchez bone bed, but there is not reliable evidence that Dr. Dickson's *os innomanatum* belongs to this bed. It is a case open to investigation without prejudice, like the early discoveries of Boucher de Perthes.

Sir Charles Lyell's profile at Vicksburg, Mississippi, eighty miles north of Natchez, 1846.

1. Below low water, cretaceous.
2. Sand-bed with rolled pebbles, same as at Natchez. [This he regards as Eocene tertiary, but possibly drift. No tertiary has been reported by American geologists along the river above Vicksburg.]
3. Calcareous loam or loess, same as at Natchez.
4. Soil.

The loam, or loess of the Mississippi, has since 1846 been traced by Professors Safford, Owen, Swallow and Worthen, from Vicksburg to the mouth of the Missouri, and up the Ohio beyond the Wabash, where it contains the Megalonyx.

WORKS OF ART, GRINNELL LEADS, KANSAS.

An instance is given by Professor Daniel Wilson (Prehistoric Man, p. 46) of a flint knife, found at the Grinnell leads, Kansas, by Mr. P. A. Scott, at the depth of fourteen feet. It is probable that this knife belonged to some of our Indian races, with whom flint implements predominated, but how ancient the overlying deposits are, cannot be considered as well settled.

There is, however, a case given, upon the authority of Professor Agassiz, where the jaws, teeth and bones of the human frame were found by Count Pourtales in a calcareous conglomerate in Florida, which is geologically recent, but which they consider required ten thousand years to accumulate over the bones.* As no opinion is given as to the race to which these remains belong they cannot be connected with either the red man, or the mound builder. They may be parts of a skeleton, drifted from far distant lands, especially from the islands which constitute the West Indies. Unfortunately there is not only

*Count Pourtales, in reference to several erroneous accounts that have been given regarding the Florida bones, makes the following statement in the "American Naturalist," Vol. ii, p. 443 (Oct., 1868). "The human jaw and other bones, found in Florida by myself in 1848, were not in a coral formation, but in a fresh-water sandstone on the shore of Lake Monroe, associated with fresh-water shells of species still living in the lake (*Paludina, Ampularia, etc.*). No date can be assigned to the formation of that deposit, at least from present observation."—EDITOR.

in this but in all the cases quoted in the United States, pertaining to the quaternary period, a degree of uncertainty in the evidence, which is fatal to a scientific result.

The human skeleton described by Dr. Dowlais, found at the depth of (16) sixteen feet in the city of New Orleans, to which a high antiquity has been ascribed, belongs, according to the later investigations of Professor Hilgard, of the survey of Louisiana, to the recent alluvium.

While the canal around a rapid of the St. Lawrence was being excavated near Brockville in Canada, Dr. J. Raynolds of that place procured several copper tools, which are reputed to have been found (14) fourteen feet below the surface (Smithsonian Contributions, vol. x, p. 208). There were at the same depth a number of human skeletons, placed there evidently, according to some form of sepulture. The copper spearheads found among these skeletons correspond in their general characters to those of the mound builders, except in the mode of attachment to the shaft. In the Brockville spears there is a pointed spike or shank, instead of the usual hollow socket. There was also a spade-like tool of copper, of which none have been found in the mounds, but I have seen one from the drift gravel of Lake Superior closely resembling it. Neither have the copper spearheads, attributed to the mound builders, been found at such depths in their works as to render it certain they were made and used by them. The age of the Canada formation is not determined, but appears to be of the later drift, or terrace period, and the remains may have received a part of their covering from the alluvial wash of higher lands, or from slides. This locality requires farther examination.

On the Ashley river, near Charleston, South Carolina, Dr. Holmes reports an instance where he discovered fragments of pottery at the bottom of a peat-bog, in close connection with remains of the Mastodon and Megatherium (Proceedings of the Philadelphia Academy of Natural Sciences, July, 1859). There is here a close resemblance to the pottery finds of the deposits of the Nile, and the relics of the peat bogs at Abbeville in France. There is in them evidence of high antiquity, not, however, carrying us beyond the alluvium.

As the pendant or "plumb bob" of Sienite procured by Professor Grimes at the depth of (30) thirty feet below the present surface, in the bed of an ancient lake in California, will doubtless receive a notice in these "Proceedings," from Colonel Foster, I make no farther reference to it than to call attention to the fact that the deposit must be regarded as more recent than the drift. Mr. John Collot, of Vermilion County, Illinois, has in his possession a similar ornament or implement three inches long, made with great care and symmetry from a piece of pure crystalline specular iron ore. He has also a fragment of another precisely like the above, and a beautifully formed hemisphere, like a paper weight, fabricated of the same material. They were found on the surface about fifteen miles south of Covington, Indiana.

Near Perryville, in Knox County, Ohio, about twenty years since, I. N. Pillsbury, Esq., Civil Engineer, of Cleveland, Ohio, discovered a "plumb bob," very like the Sienitic one of California. It was taken by him about one foot beneath the surface, within one of the ancient forts common in Ohio. The material is a whitish gray crystalline limestone, not as elegant in form as those of Mr. Collot and Professor Grimes, but about the same length.

In all of them, the hole at the upper end through which they were suspended, tapers towards the centre. It is more likely they were amulets or ornaments worn about the person, and are of an era subsequent to the mound builders.

In this epitome of the evidences of man in the United States prior to the historical period, I have not alluded to the reputed skulls of Calaveras County, California, produced respectively by Professors Whitney and Blake. In regard to both of them there is a direct contradiction in reference to the authenticity of the relics, and the age of the deposits in which they are supposed to have been found. These "Proceedings" will no doubt contain their views and their proofs at full length. In every instance where we descend below the alluvium in search of human remains and relics we are thus far met by conflicting testimony as to the facts.

The later clay, sand and gravel beds of the drift era on the lakes, and the newer drift and loess beds of the Mississippi, should contain them, if these deposits are, as they appear to be, coeval with those of the Somme. In the Belgian caves we are also furnished with works of man that seem to be as ancient as the closing out of the glacial period.

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2. Archæology of the Mississippi Valley. By W. De Hass, of St. Louis.

The progress of Archæology has been rapid and successful abroad, but at home little comparatively has been accomplished. In the great valley of the Mississippi *nothing* had actually been done previous to the present season.

Anxious to compare the ancient earthworks of the Mississippi, Ohio, and subordinate valleys, I came West in March, 1868, and as early as practicable commenced operations.

The extensive system of mounds on the American Bottom having long attracted attention, and there being some diversity of opinion as to the character of these interesting monumental remains, I have examined with care these really fine and remarkable works, and respectfully submit, for the consideration of members of the Association, the result of observations and explorations thus far prosecuted.

These researches have been prosecuted in that spirit of scientific inquiry which is pressing forward exploration in other lands; which is not content to rest with mere affirmation or specious speculation, but demands careful, thorough, and laborious efforts to develop the primordial life of man, trace out his remains of art and industry, collect the fast perishing memorials, and otherwise preserve these early monuments and vestiges of art.

The American Bottom, Illinois, presents the most extensive and remarkable system of ancient *tumuli* to be found in North America. Other parts of the country present more varied works, but nowhere are the mounds so large and numerous, and arranged with so much system, as the locality referred to. They exhibit, indeed, a city of mounds, a vast and mysterious collection of monumental remains.

Familiar with the earthworks of the United States, I have been not a little surprised at finding those on the opposite side of the Mississippi, entirely tumular. Elsewhere they generally occur in connected groups, associated with circumvallations, ramparts and mural works.

Before entering on a description of these interesting monumental remains, with vestiges of art found in and adjacent to them, I will give a brief historical retrospect of the great alluvial known as the "American Bottom." This comprises the most extensive and valuable alluvium in the United States. It stretches from nearly opposite the mouth of the Missouri to the Kaskaskia, over eighty miles, and has an average breadth of about seven miles. Of inexhaustible fertility; abounding in scenery highly picturesque—lakes, creeks, and running brooks—it was evidently the favorite abode of an active and populous community in prehistoric days. The most desirable portion is that lying north and east of Big Lake, or the Belleville road. Much of this is prairie, and the early occupants of the valley selected this as their home, built their mounds and made secure their foothold on the broad domain. The name by which this fine body of land is now known was derived from a settlement chiefly from Virginia, Kentucky and Tennessee, many of them followers of George Rogers Clark. These were *Americans*, and known as such in contradistinction to those of Gallic descent.

At an early day the indefatigable explorers, missionaries and traders, from *La Belle France*, penetrated the valley of the Mississippi and planted the standard of the cross among the native tribes whom they found occupying the country.

When La Salle descended the Mississippi in 1686, the principal native tribes occupying the region embraced within these observations were the Cahokias, Tamaroas and Kaskaskias, belonging to the Illinois confederacy. Allouez soon after founded his mission among the Kaskaskias, and about the same time Gravier and Pinet claim the credit of founding missionary posts with the Caoquias.

Sebastian Rale (Biography in Sparks) visited the Missions in 1692, spending two years among the Illinois Indians. Charlevoix visited the region in 1721, and speaks of the Caoquias and Tamaroas.

These early writers describe the manners, habits and customs of the Indians, *but are silent about the mounds.* The natives were indolent and dirty, devoted to hunting and fishing. They were acquainted with some of the simpler arts. Charlevoix describes the mode of making thread for sewing their robes, moccasons, etc. "After stripping the flesh from the sinews of the roebuck they expose them to the sun for the space of two days; after they are dry they beat them, and then, without difficulty, draw out a thread as white and as fine as that of the Mechlin, but much stronger."

One of the earliest reliable writers on the antiquities of the American Bottom was Breckenridge. In his "Views of Louisiana," published in 1817, interesting sketches are given of his visit to the Indian and French villages at Kaskaskia and Cahokia. He speaks of a "number of large villages; that a lucrative fur trade was carried on, and their agriculture extensive." "These settlements sent to New Orleans in one year (1746) 800000 pounds of flour, while at the time there was not a single settlement on the western side of the river." The principal villages were Kaskaskia, Prairie du Rocher, Cahokia, De Charte, and St. Philip. The two last have entirely disappeared." (1812-13.)

Breckenridge visited the mounds. He says: "As I approached the foot of the largest mound I was struck with a degree of astonishment, not unlike that which is experienced in contemplating the pyramids."

The Indians disavowed all knowledge of the monuments. They had no tradition concerning their origin or purposes. The old chief of the Kaskaskias told Mr. Rice Jones that in the war of his nation with the Iroquois, the mounds on the American Bottom were used as forts.

From the days of Breckenridge's observations to the present, these interesting antiquities have attracted attention but not investigation. Most books of travel were incomplete without some fanciful notice. The periodical press have repeatedly noticed them. Speculation has been busy as to the origin, character and purposes of these remarkable works, and, worst of all, ignorance, with brazen assurance, has claimed to be heard in expounding the mystery of the mounds. So remarkable, varied and extensive are these monumental remains that many persons could not comprehend how human labor could have erected them. The Pyramids would doubtless still more baffle and confound these sagacious observers.

A fluviatile origin has been a ready and plausible theory with many persons. Another respectable authority regards them as outliers of loess brought to their present form by denudation.

The writer of an elaborate paper in the "Edinburgh Review," speaking of the great mound says: "This mound is constructed with as much regularity as any of the *teocallis* in the South, and was originally *cased with stone* (some American archæologists maintain *with brick*) and surmounted with one or more buildings," etc. The writer does not inform us who these "American archæologists" are, but possibly they are allied to that class who "maintain" the "geology of the *tumuli*," prate about "stratified mounds," and delight to use scientific terms, the very meaning of which they are utterly ignorant. These are third-rate country doctors, rural district teachers, and other tyros who imagine they have discovered in graphic mica, or encrinital limestone, genuine Hebraic inscriptions.

It is not surprising that novices should commit egregious blunders in attempting to discuss subjects they do not understand; but it is surprising that those whose position and investigations should have induced them to examine carefully the character of these works before expressing positive opinions, have failed to do so. The only charitable conclusion is they never examined the mounds. No man whose opinions are worth quoting could have examined even one of these interesting monuments, and not declared, unequivocally, in favor of artificial origin. The proofs are clear, abundant and conclusive. Externally and internally, character, structure, position and contents, all incontestably prove them the work of man's labor, industry and spirit of combined action. All, from the largest to the smallest, are the result of human agency. On this point there need be no farther cavil or doubt.

With all due deference to very respectable authority in the West I must dissent from his expressed views that these mounds are *loess*. They are totally different in character and composition. The *loess* is uniform buff colored; all mounds alluded to *are a heterogeneous mottled composition* of vegetable mould, sand and clay. This is the unvarying character of every mound on the American Bottom that I have examined. The great tumulus presents this marked feature, as do the smallest composing this extensive and remarkable system.

But of the mounds—who erected them? How were they constructed? When built? For what purpose designed? Whence came and whither went their builders? These are questions which have baffled inquiry in other days, and still defy our keenest ken. Were their builders Autochthones? or were they immigrants from other lands? How derived their knowledge of agriculture, of working stone, of pottery and other useful arts? How lose all these?

Impressed with the importance of giving these investigations all the attention possible I have labored assiduously to this end. I have traversed the field hundreds of miles, over lakes, across bogs, up creeks, down streams, penetrated its geological strata and climbed long miles of tortuous bluff; have examined, located, measured and mapped over one hundred and fifty mounds, excavating many, and collecting several hundred specimens of ancient art, representing the stone age, the fictilia, the art and skill of the mound builders. This has not been unattended with labor, exposure and expense. But I have the gratification to know that the question of the mounds —whether natural or artificial—has been forever settled. These explorations also afford another pleasure: they enable the archæologist to read from the great book spread out in the mounds and their contents much of the history, character, habits and customs of the people who occupied these broad valleys in long anterior years. And yet the work has scarcely begun. A vast field for scientific exploration opens to the enlightened energies of the Government, of societies and individuals. It is indeed discreditable to our intelligence as a people that these explorations were not made years ago. Many of the finest monuments of antiquity, here and elsewhere, have already disappeared, and the hand of vandalism is being laid violently upon others. The effacing agencies of time and man are rapidly consigning many fine works to irretrievable destruction. Shall this condition of things continue? Better allow the English Societies, as suggested by a writer in the "Edinburgh Review," to organize a commission and explore these American monuments; or, as has been suggested in Paris, that the Emperor send a commission to explore the mounds and carry off the implements of industry and of the chase, the weapons of war, etc., which may be discovered. Far better this than that these fine works shall go to destruction and their vestiges of art be scattered and utterly lost.

Two grand groups of ancient *tumuli* loom up on the broad surface of the American Bottom. They are distant from the central figures about six miles, but connected by a series of smaller mounds, forming a continuous chain, and constituting one grand and extensive system of tumular works — unequalled for size, number and interesting feature on either the sub-continents of America.

One of these groups stands within the city limits, and adjacent to East St. Louis; the other six miles to the north-east, lying chiefly north of the Ohio and Mississippi Railway. These are connected by a series of tumuli, stretching along Indian lake and Cahokia creek, the entire system, including those along the bluff numbering over two hundred mounds.

These, collectively, present a vast city of mounds in ruin. They undoubtedly constituted the seat of a great power — a community, little less populous, perhaps, than that now centering within an area of twenty miles of this great modern metropolis of the West. The upper group, containing the most important monuments, was doubtless the citadel of the ancient empire. It comprises over sixty mounds, arranged with great system, and in marked position towards each other. The great mound, constituting the principal feature, is supported by four elevated squares, and numerous large tumuli of manifest importance in the system.

The mounds comprising these respective groups are conical, ellipsoidal, square, parallelogram and paræform. Some are perfect cones, others frustum. They vary in height from five to ninety feet, in some instances presenting an angle of nearly sixty degrees. They are all of earth taken from the surrounding plain or bluff, and constructed with symmetry, neatness and manifest design.

It is claimed as a noticeable fact that corresponding excavations can be observed near most of the mounds. I have noticed this quite marked in some instances, but only in localities where the vegetable mould was found underlaid with a deposit of sand. With their rude implements and facilities for removing soil the mound-builders could not make heavy excavations, but would rather avail themselves of the most readily removed.

I have failed to detect near any of these mounds the fosse so frequently noticed near Ohio valley tumuli. They correspond in general external appearance, internal structure and arrangement to the ancient tumuli of other parts of the country, except those of an elliptical type. This class occurs more frequently here than elsewhere. The square mounds find counterparts in the elevated squares at Marietta, Ohio.

I will not attempt to describe in detail this extensive system, but propose merely a general running sketch, reserving for another occasion a more particular description.

A general design is manifest in all the ancient earthworks of America. In the Ohio valley they are found in connected systems. In the Mississippi valley, or that part lying opposite the city of St. Louis, they occur alone in tumular erections, arranged in groups, with outstanding guards, system and unmistakable design.

It is difficult at the present stage of these investigations to determine the true position in archæology of these Illinois mounds. They may be older than other works in the Ohio and subordinate valleys. They certainly present a distinct type. I have not discovered in this extensive field a single circumvallation, hill or valley fortification, nor the semblance of a mural work.

The remains of art found among these mounds — stone implements, fictilia, etc. — indicate a knowledge, quite equal, if not in advance, of art remains from the mounds of Ohio, West Virginia, Kentucky, Indiana, etc. There is a decided difference between some of their stone implements which will be more particularly noticed hereafter. This fact induces the belief that they belong to different people. As to the object of the mound, without attempting to advance an hypothesis based on incomplete observations, it may be safely assumed that all mounds wherever, whenever, or by whomever constructed, were *primarily* designed as places of sepulture. This we read alike in the simple and often scarcely distinguishable tumuli in the valley of the Mississippi, or the isles of Britain, as we do in the huge mounds on the Cahokia, or the vast earthen and megalithic monuments of Northern Europe, or the valley of the Nile. They were often devoted to other uses, but the great first purpose was sepulchral. They doubtless often served a triple purpose—tomb, temple, dwelling place. The large square works possibly supported the houses of important personages, or picketed, served as places of defence. The great mound probably supported the principal temple, also the house of their cazique or king. Others served as guard posts, and still others as places of defence.

Many of the larger tumuli are believed to have contained secret entrances which were used after the structure had been completed. This was the case of the great mound at Grave Creek, Virginia, a mound near New Madrid, and it is a noticeable feature in the Pyramids. Mrs. Hill, who resided a quarter of a century on the great mound, informs me that a large entrance was at one time discovered, but filled up to prevent wild animals making therein their dens. Most of the large sepulchral mounds of the West contained originally rude chambers of wood and stone combined. The mound builders did not understand the principle of constructing the arch. In no instance has it been discovered in ancient ruins north of Mexico. The nearest approach was the horizontal or overlapping arch. As the great mound is the largest and most important in the United States, it may be interesting to describe it briefly.

This huge structure—the largest in the United States—stands on the south side of Cahokia creek, two hundred yards from the stream, in the midst of an extensive and most remarkable group of mounds of various size and shape. It was doubtless originally an immense tetragon, supported by a heavy terrace on the south and west, approached by a talus. The destructive agencies of wind and water, uprooting of trees and modern vandalism have much defaced this vast and most interesting work.

Its present dimensions are: north base, five hundred and sixty feet; south base, seven hundred and twenty feet; summit, length, three hundred and ten feet; breadth, one hundred and forty-six feet.

The north side is much the most precipitous. The terrace approaches from the south and west, and is in depth one hundred and twenty feet. The talus approaches from the south, is fifty-five feet broad at top, one hundred and twenty feet in length, and one hundred and twenty-five broad at base. Perpendicular height of mound, as nearly as can be ascertained, ninety-one feet. It was originally, even within the historic period, considerably higher. The base covers nearly six acres. The solid contents have been roughly estimated at twenty-five millions cubic feet. The Pyramid of Cheops covers nearly thirteen acres.

The great mound was originally surmounted by a conical mound ten feet in height. This peculiar feature characterizes one of the square elevations west of the great tumulus. Removal of the small mound revealed numerous interesting relics — human bones, stone implements and weapons, vases of unburnt earthen-ware, etc. These, with a large collection of ancient art were long kept by Mr. Hill, former proprietor of the mound.

The material composing this imposing work, corresponds with that entering into the smaller mounds—vegetable mould, clay and sand. Wherever cut into, and numerous excavations have been made for well, cellar, cistern, ice-house, etc., the same unfailing features are present — a heterogenous mixture of humus, sand and clay, as would occur in removing the surface of lowlands and making slight excavations into the underlying strata. The earthworks of the West have been built in this manner, and when placed on or near alluvium, always show the mottled composition. The great mound at Grave Creek, Va., strikingly illustrates this feature.

The former proprietor of the mound under consideration sunk a well from the steppe, and throughout its entire course threw out broken pottery, fragments of flint, etc. At twenty-five feet, charcoal and partly burnt wood (paw-paw) were discovered. At the horizon of the surrounding plain he passed through a distinct line of dark carbonacious matter which was clearly the original sward upon which the great tumulus had been erected. Precisely similar indications were noticed by those who excavated the Grave Creek mound and which can be seen at this day.

It may be remarked that water was not struck in this well above the ordinary level, as has been alleged by those who would disprove the artificial character of the mound.

This gigantic tumulus was defended by four elevated squares placed respectively, one on the east, two on the west, and one on the south-west. They vary from twenty to thirty feet in height, and from two hundred and fifty to three hundred feet square. The one on the east is defended by a mound at the south-west corner, and one of those on the west is surmounted by a mound similar to that which crowned the great tumulus.

This imposing monument is popularly known as "Monk Mound;" a decided misnomer, applied because a family of the order of Trappists, located in the vicinity in the early part of the present century. They did not, however, occupy the great mound, but dwelt upon one of the elevated squares immediately west of the larger structure. The author of "Views of Louisiana" visited these rigid religionists about 1813, and thus refers to them: "They at present occupy four or five cabins built upon an Indian mound about fifty yards [feet] high and one hundred and fifty feet *square.*" . . . "The other buildings, cribs, stables, etc., ten or fifteen in number, are distributed on the plain below." . . . "They number about eighty persons. It is about a year since they fixed up this place. They had a kitchen garden on the apron of the great mound." The Trappists remained on the American Bottom until 1819, when, after considerable reduction in numbers by death, they quitted the place forever. The mound, with four hundred and twenty acres of adjacent land, was purchased by T. Ames Hill, a native of Massachusetts, but long a resident of Kentucky and St. Louis. He moved on the property in 1831 and erected a cabin upon the very summit. He there lived many years, died and lies buried at the north-west corner of the great tumulus. From his widow, who still survives in vigorous health at the age of seventy-five, I have derived many interesting facts. She resided twenty-five years on the mound, and now lives near Collinsville.

Of the other mounds comprising this interesting group it will be impracticable, in view of the space already occupied, to more particularly describe them. Some are quite remarkable for size, character and design; several are evidently located as defensive works to the great mound. The whole present an admirable system, and sensibly impress us with their extent, grandeur and the spirit of combined power which influenced their builders. Nothing but a diagram can properly represent their position, connection and arrangement. This, I regret, that pressing personal engagements will prevent my preparing in time for the Association. When brought under the instrument, instead of standing without relation, they are found perfectly in line, or in position to each other. The bluffs are also found lined with tumular erections, and everywhere show they have been used as burial places. The common red man may have availed himself of these commanding points, as he did of the mounds, but still there are evidences of more ancient occupancy. Some of the bluff mounds are quite remarkable. The Sugar Loaf, in Madison county, stands perfectly isolated upon the verge of an elevated plateau, commanding from its lofty summit a view of perhaps forty miles. This structure is a sharp cone, over forty feet in height, at an angle of nearly sixty degrees. A similar work, bearing the same name, looms up upon the bluff in Monroe county. Between these distant points a great number of other mounds occur, some quite high, and generally commanding extensive views. Truly, we are in the midst of a grand system of prehistoric remains!

I have said that the mounds were intended primarily as a place of sepulture; but it must not be understood that all mounds were so used, or that they were places of general burial. They were memorials to important personages, and often, doubtless, family sepulchralia. Western tumuli may be classified as mounds of *sepulture*, mounds of *defence*, and mounds of *observation*, besides those of a miscellaneous character. All were frequently used for burial. Mounds also served a double and triple purpose. The masses were not buried in mounds. The bluffs and lowlands in the vicinity of the tumuli show extensive cemeteries. In the vicinity of the great mound, human remains, pottery, implements, etc., are dug up almost everywhere, at the depth of two to four feet.

Burial was practiced by the early nations on the Mississippi in at least three different modes; *inhumation*, by placing the body extended at length in a cist, either stone-lined or plain; *inhumation*, by placing the body erect, or in a sitting posture; and *cremation*, or burning the body and placing the ashes and carbonized bones in an urn. Parts of large vessels, which, from their position and other circumstances, were clearly funereal or cinerary urns, have been discovered in the course of these explorations. I hope yet to discover entire urns in the progress of these researches. These modes of burial were observed alike in the mounds and elsewhere.

Minor remains of ancient art constitute an instructive feature in these explorations. I have a large number of specimens, collected from mounds and other ancient depositories, or from the vicinity of them. These represent the stone age, as Scandinavian archæologists divide the progress of human labor. Metallic remains have not as yet been discovered in the mounds of the American Bottom. Copper, in its native state, was the only metal known to the mound era.

The builders of the works under consideration had attained great excellence in working stone. Some of the specimens obtained are of exquisite workmanship and faultless design. It is marvellous how, with their rude tools, they could have fabricated these fine specimens. The chisels and heavier implements, of which I have discovered some quite anomalous, are of granite, hornblende, nephrite, and other varieties of the hardest amphibolic rock. The arrowheads and spearpoints, knives, fleshing instruments, etc., are of quartz of almost every style, from the dull black of the common chert through all the gradations, up to the most beautiful opalescence of the chalcedonic varieties.

One type of these implements is most remarkable. These are *agricultural*, proving beyond doubt that the people who used them tilled the soil. Two distinct styles prevail; one long, like the blade of a spade, and the other identically our modern hoe, the eye being substituted by a double notch. These vary in size, the longer from six to fifteen inches, and four or five inches broad; the other about the diameter of an ordinary garden hoe. These implements show usage, the parts entering the soil being highly polished, such as nothing would so readily effect as attrition in sand and loam. These implements are quite anomalous; nothing of the kind having been discovered in the Ohio valley, or, so far as I am aware, east of the Mississippi, or in Europe.

A variety of other flint implements have been discovered, unlike any heretofore found. Also, a large granite implement which may have served for dressing hides or crushing corn. I notice, however, a total absence of steatite and serpentine ornaments and amulets, so common in Ohio valley mounds. The implements, ornaments and weapons in stone, indicate two classes — one representing the *palæolithic*, or undressed stone age! the other, the *neolithic* or polished stone age of Sir John Lubbock.

The fictilia from this important antiquarian field is varied and extensive. Vast quantities have been ploughed up and otherwise developed since the first settlement of the valley, but, sharing the common fate of such accidental discoveries, have not been preserved.

The ancient potter in the valley of the Mississippi was but little inferior to his brother of the plastic art in the valley of the Nile. The wheel was not in use; the ware was all hand-made, and sun-dried, or indifferently baked. Two or more varieties of manufacture have been discovered. The material is a breccia of clay and pounded muscle-shell; but, in some instances, the shell has been substituted by small rhomboidal fragments of white spathic carbonate of lime. The ware is of irregular thickness, but exhibits much tenacity, and is capable of resisting irregular dilatation or excessive shrinkage. The ornamentation is plain, neat, sometimes tasteful, but generally rude, consisting of lines, dots, chevrons and zigzag patterns.

The vessels are urns, vases, cups, dishes, etc. The handles of some are tastefully made in imitation of the wolf, fox, etc. A farmer residing near the Great Mound, excavating for an ice-house in a mound, threw out over a dozen handsome vessels, mostly ornamented with the animal-shaped handles. Many of the mounds contain vessels of the style alluded to. Some of these vessels doubtless, contained viands for the sustenance of the departed; while others were tributes to the worth of the lost, as was customary with many semi-barbaric people. It is a fact worthy of mention in this connection that the oldest specimens of British pottery are principally funereals found in the tombs of the ancient Britains — their barrows and cairns. Some of the finer specimens discovered in the progress of these explorations, show a polishing or glazing, leaving minute striæ, as if done with a tuft of grass dipped into a *barbotti*. Other specimens exhibit handsome and bright coloring.

Without advancing a theory as to the origin of these mounds, or whether the builders were contemporary with those who erected the earthworks in Ohio and to the south-west, I will simply remark, that in all probability, diverse peoples occupied these valleys, and probably these very monuments, in prehistoric times. We have the positive evidence of at least three distinct peoples using and burying in some of these mounds: the actual builders, the Indians and the white man. One of the elliptical mounds in East St. Louis is covered with graves of the early white settlers. In some British barrows, the Pict, the Roman and the Saxon lie entombed in their relative positions. In the mound just alluded to, which is to be removed during the present month, the interesting archæological fact will be exhibited, of three distinct classes of interment in the same *tumulus*.

It was intended to describe more fully the process of exploring these ancient mounds; the modes of construction; the character of their contents and other peculiarities; also to notice the fauna of the mound period, as shown by the mounds and ancient localities, but the time allotted to this paper obliges me to defer these and additional particulars.

These researches afford us a vivid picture of the habits, customs, social and artistic advancement of the prehistoric peoples who occupied the broad valleys and fertile plains of the West. The great carnivora had not yet disappeared from the valley of the Mississippi, and the huge extinct pachyderms were sufficiently familiar to the Aboriginal artist to be faithfully depicted. These people subsisted on game, fish, fruits and maize.

Were they one or many? Autochthones, or immigrants? As crania constitute the truest indication of race, it is possible these researches may determine this interesting question of the early races at the West.

That more than one dwelt here, previous to the red man, who can doubt? How many times this great valley and the entire continent has been inhabited and re-peopled, previous to the present, who can even remotely conjecture? The mounds and vestiges of ancient art, clearly indicate different peoples, either cotemporaneously, or at separate epochs before the dawn of history.

We have not reached that advance in science attained by the anthropologists of Great Britain, who pronounce the character of crania from the form of the tumuli in which they occur.

These researches have not yet determined the interesting anthropological fact, if a fact it is, that *long* mounds contain *long* or *dolichocephalic* skulls; and *round* or conical mounds, cover a round-headed, or *brachycephalic* race.

There are, in the extensive and interesting groups on the American Bottom, fine specimens of these separate types, but it is scarcely probable exploration will develope the peculiarities alluded to.

It is proposed to prosecute these researches with energy, during the present season, embracing the entire valley, or, so much of it as is known as the American Bottom. They promise the most valuable results to science. There are a number of mounds in each group, of sufficient importance to demand careful and thorough exploration. The great tumulus of the series, or Monk Mound, should be carefully excavated. But this would require time, patience, and considerable means. Permission has been obtained to make the exploration, but the work is too heavy for individual enterprise. Cannot some measures be adopted by the American Scientific Association for thoroughly prosecuting these researches? The matter is submitted to the wisdom and judgment of the members.

3. Report on Archæology and Ethnology. By W. De Hass, of St. Louis.

The Chairman of a Special Committee on American Archæology and Ethnology, appointed at the Baltimore meeting, having been prevented by the war, and other circumstances, from presenting the result of the Committees' operation, begs leave, respectfully, to submit the following Report.

The Committee from the first labored under many disadvantages, and serious embarrassment, for want of funds. The anticipated aid from the Government and Societies having failed, the Committee alone relied upon private liberality for means to prosecute the work committed to their charge. A comparatively small fund having been realized from private sources, the system of research which had been adopted was prosecuted with industry and success. The work principally devolved upon the chairman; he laboring without remuneration, and most of the time at his own expense. Notwithstanding these discouragements much has been accomplished. Some of the most interesting localities in the United States have been visited; thousands of earthworks, mounds, circumvallations, etc., examined; specimens of ancient art and industry collected; numerous mounds excavated; extensive groups measured, mapped and described. The result of researches thus far prosecuted have been altogether satisfactory.

The Committees' functions having been suspended during the war, their Chairman can only report up to that period.

Had the Committee not been constantly embarrassed for funds, most valuable and important results would have been secured to science. Even under these disadvantages, it is believed more has been accomplished than all previous efforts to develop the Archæology of the United States.

The Chairman would respectfully represent the rising importance of American Archæology. Its progress is onward, and it is due to the cause of science that some active and energetic measures be taken to promote research among prehistoric monuments in our midst.

Whilst the enlightened Governments of Europe are zealous and active in efforts to trace out the beginning, progress, and primordial life of man, to develop his remains of art, what are we doing? In Paris, there is a well organized society devoted to the investigation of *American Archæology*. It is discreditable to our intelligence as a cultivated people, that so little attention is given this important branch of scientific inquiry.

The chairman would respectfully suggest that some decided action be taken by the Association for promoting inquiry and research. That the functions of the old Committee be either revived, or a new Committee appointed. He is ready and willing to coöperate in any way that may be desired.

He would specially recommend that measures be adopted for securing a National Museum for American Antiquities. To effect this he is willing to make large contributions. He is also willing to devote a portion of his time towards prosecuting scientific research.

He would farther urge another suggestion; that much might be effected by popularizing the subject of American Archæology through public lectures. Those competent to the work should be selected to perform it.

A vast field is open to energy and enterprise, and it is hoped that the importance of the subject will not be overlooked by the friends of science assembled at Chicago.

These views and suggestions are respectfully submitted.

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4. The New Arctic Continent, or Wrangell's Land, discovered August 14, 1867, by Captain Long, of the American Ship Nile, and seen by Captains Raynor, Bliven and others, with a brief notice of Baron Wrangell's Explorations in 1823. By William W. Wheildon, of Charlestown, Massachusetts.

In December, 1867, considerable interest was excited through the country by the announcement by telegraph from San Francisco, of the discovery of a "New Continent in the Arctic Regions," which had received from the discoverer the appropriate name of "Wrangell's Land," after the distinguished Russian explorer. The discovery was said to have been made by one of the whale ships visiting the Arctic Regions through Behring Strait, which had just returned to the Sandwich Islands. It was also stated that the season had been remarkably mild—one of the most temperate ever known—and that one of the whale ships had been able to penetrate as far north as latitude 83° 30′. The announcement that land had been discovered in the region indicated seemed probable, but the statement of the latitude reached—never having before been attained in any part of the Arctic regions—was received with some incredulity; in addition to which there was evidently an error in the statement of the longitude as received by the telegraph. To those who were acquainted with the geography of the region indicated, hardly any reported discovery of land would have been incredible, so uncertain and imperfect was the knowledge of it; but it might have been considered, as in the present case, a rediscovery, or perhaps a simple confirmation of a general belief.

When the printed accounts reached us from the Sandwich Islands, it appeared that the land reported was first seen by Captain Long, of the American whale ship *Nile*, of New London, on the 14th of August, 1867, in clear weather. The west point, which he named "Cape Thomas" (after the seaman who first reported the land), was in latitude 70° 46′, longitude 178° 30′ E. On the morning of the 15th, the ship at 9 o'clock, was eighteen miles distant from its western point. "The lower parts of the land," reports Captain Long, "were entirely free from snow and had a green appearance as if covered with vegetation. There was broken ice between the ship and land, but as there were no indications of whales I did not feel justified in endeavoring to work through it and reach the shore, which I think could have been done without much danger."

Captain Long sailed to the eastward, along the land, during the 15th and 16th, and in some places approached it as near as fifteen miles. "On the 16th the weather was very clear and pleasant, and we had a good view of the middle and eastern portions of the land. Near the centre, or about in the longitude of 180°, there is a mountain which has the appearance of an extinct volcano. By approximate measurement I found it to be 2480 feet high."

Captain Long says he had excellent observations on the 16th, and the south-eastern cape which he named Cape Hawaii, he found to be in latitude 70° 40′ north, and longitude 178° 51′ W. He adds, "it is impossible to tell how far this land extends northward, but as far as the eye could reach we could see ranges of mountains until they were lost in the distance." Captain Bliven, of the whale ship *Nautilus*, said he saw the land north-west of Herald Island, as far north as latitude 72°.

Captain Raynor, of whale ship *Reindeer*, in a letter of November 1st, 1867, to the editor of the "Pacific Commercial Advertiser," gives an account of this land, "lying in the midst of the Arctic Ocean, hitherto but little known." He says, "this land has hitherto been considered to be two islands, one of which is marked on the English maps as Plover Island, which is laid down to the W. S. W. of Herald Island. The other is simply marked 'extensive land with high peaks.' On my last cruise [the one just terminated] I sailed along the south and east side of this island for a considerable distance, three different times, and once cruised along the entire shore, and by what I considered reliable observations, made the extreme south-west cape to lie in north latitude 70° 50′ and east longitude 178° 15′. The south-east cape I found to lie in north latitude 71° 10′, and west longitude 176° 40′. The south coast appears to be nearly straight, with high rugged cliffs and entirely barren. The north-east coast I have not examined to any extent, but it appears to run from the south-east cape in a north-westerly direction for about fifteen or twenty miles and then turns to the north and north-east. I learned from Captain Bliven that he traced it much farther north, and has seen others who traced it to north of latitude 72°. I think there is no doubt that it extends much farther to the north, and that there is another island lying to the east of it, say in longitude 170° west, and to the north-east of Point Barrow, with a passage between it and the land I have just described. . . . I would add that the south-west cape of this island described above, lies seventy-five miles distant from the Asiatic or Siberian coast."

According to Captain Long's observations it will be seen that the extent of the discovered land from east to west, is 2°

39′ of longitude, equal to about fifty geographical miles; but according to Captain Raynor's observations its extent is equal to 5° 5′ of longitude, or nearly one hundred and three geographical miles, or about one hundred and twenty statute miles. So also if the statement of Captain Bliven is to be received (and there appears to be no reason why it should not be received) the extent of the land from the south-east cape (Cape Hawaii, of Captain Long) to the north as far as reported by Captain Bliven and others, must be at least one hundred miles and probably more.

The season of 1867, is reported as already stated, to have been remarkably mild. Captain G. H. Soule, of whale ship *St. George*, says "this season is the most remarkable ever known by the whalemen for the scarcity of ice and the good weather prevailing during the first and middle parts of the season. *Otoken*, a very intelligent native of Indian Point, told me they had two months south wind last winter [1866], which I think accounts for the openness of the season." Other captains report "whales very scarce." Captain J. B. Winslow, of the bark *Tammerlane*, says the weather was fine in August and September; whales scarce; took seven; of the third whale he took he speaks as follows: "The third whale we took was a stunner, and deserves special mention. It was the biggest whale, by at least one quarter, that I ever saw alongside a ship. My third mate kept a tally of what turned out at the cooler, and it yielded *three hundred and ten barrels and nineteen gallons*. It was not so fat as some we caught. I have taken whales that made two hundred and fifty barrels of oil, but never before saw one that would compare in size with this. I think it must have been one of the original whales that Noah had charge of, which has been growing ever since." *

* In regard to the reported scarcity of whales, the following statement will show how the matter stood in 1867, as compared with the previous years:

The number of vessels refitting at the Sandwich Islands in 1867, was	75
Having sperm oil in barrels,	1940
" whale " " "	52050
Averaging to each vessel, in barrels,	720
Total amount of whale-bone,	11313 lbs.
Number of vessels in 1866,	76
Average number of barrels to each,	680
Total amount of whale-bone,	10980 lbs.

Showing an excess of forty barrels of oil to each vessel, in 1867 over 1866, and an excess of whale-bone.

Baron Wrangell, in his explorations along the coast of Siberia, in March, 1823, was visited in camp by a Tschuktschi chief, whom he represents as "a very civilized person in his way," and who drew for him with a piece of burned wood, an outline of the coast in the neighborhood of Cape Schelagskoi. "He farther assured us," says Baron Wrangell, "in the most positive manner, that there was no other island along the coast. When I asked him whether there was any other land to the north beyond the visible horizon, he seemed to reflect a little, and then said that between Cape Schelagskoi and Cape North, there was a part of the coast where, from some cliffs near the mouth of a river, one might in a clear summer's day descry snow-covered mountains at a great distance to the north, but that in winter it was impossible to see so far. He said that formerly herds of reindeer sometimes came across the ice of the sea, probably from thence, but that they had been frightened back by hunters and by wolves; that he had himself once seen a herd returning to the north in this way, in the month of April, and that he had followed them in a sledge drawn by two reindeer for a whole day, until the rugged surface of the ice forced him to desist. His opinion was that these distant mountains were not an island, but an extensive land similar to his own country. He had been told by his father that a Tschuktschi elder had once gone there with a few followers, in large *baidars*, or boats made of skins, but what they found there, or whether they ever returned, he did not know. Still he maintained that the distant northern land was inhabited, and adduced as a proof of it that some years ago a dead whale had been found at Arautan Island [on the coast near by], pierced by spears pointed with slate;* and as the Tschuktschi do not use such weapons, he supposed that the whale must have been wounded by the inhabitants of the northern land." Baron Wrangell's position when he received this information was in latitude 70° 3′ and longitude 171° 3′ East.

A few days after this, Baron Wrangell started with sledges over the ice, with a view to reach or discover this northern land. Following along the coast for a day or two, on the 13th of March, he left the shore and proceeded in a N. N. E. direc-

*The people of the Aleutian Islands use similar spear heads it is stated.

tion. With the usual difficulties of cold, wind, storm and ice-hummocks, he continued on until the 23d, about ten days, having sent his sledges back excepting two. According to his reckoning he reached latitude 70° 51′, longitude 175° 27′. His distance from the coast was one hundred and five wersts or a little more than sixty geographical miles. At the extreme point of his journey, Baron Wrangell says, "we climbed one of the loftiest ice-hills, affording an extensive view towards the north, and from thence we beheld the wide immeasurable ocean spread out before our gaze. It was a fearful and magnificent spectacle. Fragments of ice of enormous size were floating on the surface of the agitated ocean, and were dashed by the waves with awful violence against the edge of the field on the farther side of the channel before us. . . . With a painful feeling of the impossibility of overcoming the obstacles which nature opposed to us, our last hope now vanished of discovering the land which we still believed to exist; and we saw ourselves compelled to renounce the object for which we had striven through three years of hardship, toil and danger. We had done, however, all that duty and honor demanded; and any farther attempts being totally hopeless, I determined to return." At this time being in latitude 70° 51′, longitude 175° 27′ E., Baron Wrangell was less than sixty miles from the westerly point of the discovered land as reported by Captain Raynor.

A few days later, early in April, 1823, Dr. Kyber, companion of Baron Wrangell, became acquainted with some Indian chiefs, who spoke much of a "more northern land, the lofty mountains of which were visible on very clear days from a place which they called Jakan, and which they described with tolerable minuteness." On the 8th of the same month Cape Jakan was reached and found to be in latitude 69° 42′, longitude 176° 23′ E., by reckoning from the previous day. "We gazed long and earnestly on the horizon, in hopes, as the atmosphere was clear, of discerning some appearance of the northern land which the Tschuktschi affirm that they have seen from this place, but we could discover nothing of it." Subsequently M. Matiuschkin made another attempt, with the consent of Baron Wrangell, to reach the northern land over the ice, with three sledges and provisions for fifteen days. He started from the shore in latitude 69° 28′, longitude 177° 44′ E., on the 9th of April, but succeeded only in getting about sixteen wersts or about eleven miles from the coast, on account of the breaking up of the ice and the many open spaces.

Captain Kellett, of H. B. M. ship *Herald*, in July, 1849, having passed through Behring Strait and standing along the margin of the ice, discovered a group of islands in latitude 71° 20′, longitude 175° 16′ W., one of which he landed upon and named Herald island. Another island which he reported has not since been seen. Captain Raynor, it will be seen, reports another island to the eastward of Herald Island, in latitude 70° W. N. W. from Point Barrow. The Russian navigator, Sergeant Andreyer, it is stated, reached some land off the coast of Asia, in 1762, which he reported to be inhabited by a people named Kraihaï.

SUMMARY.

1. Captain Long's positions, 1867,	
West Point, Cape Thomas,	Latitude 70° 46′, Longitude 178° 30′ E.
South-east Point, Cape Hawaii,	Latitude 70° 40′, Longitude 178° 51′ W.
2. Captain Raynor's positions, 1867,	
Extreme south-west Cape,	Latitude 70° 50′, Longitude 178° 15′ E.
South-east Cape,	Latitude 71° 10′, Longitude 176° 40′ W.
3. Captain Bliven and others traced the coast northward to	Latitude 72°.
4. Baron Wrangell's farthest, in 1823, over the ice,	Latitude 70° 51′, Longitude 175° 27 E.
and did not see the land.	
5. Captain Kellett's position, 1849,	Latitude 71° 20′, Longitude 175° 16′ W.

Since the preceding pages were written, a new edition of the map entitled "Behring Sea and Arctic Ocean, from surveys of the U. S. North Pacific Surveying Expedition, in 1855, by Commander John Rodgers, U. S. N. commanding, and from

Russian and English authorities," has been issued from the "Hydrographic Office, U. S. Navy," with "additions to July, 1868." The land seen and named by Capt. Long, is laid down upon the map in accordance with his statements; and a drawing is also given of the land, with its elevated points, probably from a sketch made by Capt. Long. "Wrangell's Land," we may here remark, falls about ten degrees westerly of the present boundary of the United States in the latitude of 70°.

The new boundary of the United States passes through the middle of Behring Strait, between the Diomede Islands, one degree south of the Arctic Circle, leaving Ratmanov Island on the Russian side and Kreusenstern Island on the American side. From this point it reaches towards the North Pole on the meridianal line of 168° 50′ west longitude from Greenwich. From the Diomede Islands it runs in a nearly south-west direction in the North Pacific Ocean, to latitude 50°, in longitude about 168° east. So that, at the present time, the territory of the United States extends in an east and west direction from longitude 67° west (at Eastport, Maine), to longitude 168° east in the North Pacific Ocean (beyond the most westerly island of the Aleutian Group), equal to 125° of longitude, which on the parallel of 45° north latitude would be equal to about five thousand three hundred and three geographical miles, or six thousand one hundred and eighty-seven statute miles. The position at the mouth of Columbia River is nearly in the middle of this line and the central point east and west of the United States territory, it being about the same distance from Eastport to the mouth of Columbia River that it is from the Columbia River to the most western extremity of the Aleutian Islands.

Although the newly discovered land does not fall within the enlarged limits of the United States, the honor of the discovery of it belongs to our countrymen; and the act of Captain Long in giving the name of Baron Wrangell to it, although it was never seen by him, is a well-deserved compliment to that intelligent and indefatigable explorer and to his country. The propriety of the name is generally admitted, and it has been promptly, in the first instance, adopted by our government in its first official publication—the map above mentioned—and it will no doubt be universally accepted.

5. The Habitable Features of the North American Continental Plateau, near the Line of the Thirty-fifth Parallel of North Latitude. By C. C. Parry, M. D., of Davenport, Iowa.

The recent rapid progress of settlement in the elevated interior district, properly characterized as the great Western plateau of the North American continent, is suggestive of some general principles of civilized development which may aid us in estimating future progress in particular portions of this district, especially favored by their peculiar location, climate, or natural products, as inviting and retaining a permanent population.

The aboriginal population offers the most direct means of forming a general estimate of the desirable habitable features of particular regions, though these obvious views will necessarily require correction and modification to correspond with the marked differences between the widely distinct classes of civilized and uncivilized life.

Still, other things being equal, the country which, in its ancient monuments, or its present existing races, shows traces of superior condition, large population, or conveniences of living, will necessarily indicate at least an enlarged capacity for the growth and support of progressive modern civilization.

These indices of capacity for sustaining an advanced order of civilization are especially marked near the line of the thirty-fifth parallel, north latitude. This includes in New Mexico and Northern Arizona, the favorite homes of the industrious, semi-civilized, Pueblo Indians. It is also the country especially marked by the scattered remains of an ancient civilization, exhibited in the ruins of populous towns, wisely selected with reference to favorable agricultural districts and broken pottery, showing considerable progress in the domestic arts.

The present condition of the native population, but slightly different from that noted by the early Spanish explorers three hundred and fifty years ago, includes three distinct classes; according to the relative preponderance of each of which, determined in great measure by the external features of the country, its true habitable features is brought clearly to view.

1. There is the roving Apache, with no permanent home, living by theft, and only supplementing this irregular mode of existence by the scanty native product of the soil, or the precarious returns of the chase. Without beasts of burden or domestic animals of any kind, except such as are captured in their predatory excursions and subsequently consumed for food, they roam in scattered bands over a wide section of the country, occupying the rocky recesses and inaccessible canons of the mountain districts, from which, eluding pursuit, they can most successfully prey upon the unsuspecting traveller, or accomplish their thieving raids on the cultivated fields of their industrious native compeers.

2. We have the Navajo tribes, showing an advance towards civilization in the rearing of flocks and herds, upon which they rely mainly for subsistence and clothing. Necessarily connected with this more settled mode of life, we note some considerable progress in the domestic mechanic arts, such as weaving, basket making, etc. Also some attention is given to the cultivation of the soil, and raising fruit to supply the place of the scanty native productions. Still, however, in accordance with their roving propensities, they are addicted to hunting and plunder when it can be safely carried on at points remote from their settled habitations. The Navajo country comprises the picturesque region lying west of the Rio Grande, including an excellent grazing district, and a rocky section with deep canons, clear streams and limited fertile valleys. From this desirable district a large part of the Navajo tribe have been recently removed to a Government reserve, in South-eastern New Mexico, by a very questionable policy, depriving these Indians of their natural means of legitimate subsistence, and supporting them at Government expense in an idleness that can only result in their speedy deterioration, and ultimate extermination.*

3. In the rank of ascending civilization we have the Pueblo Indians, living in defensive towns, relying for subsistence on the cultivation of the soil, and the rearing of domestic animals; industrious, hospitable, fond of trade, ready to adopt any of the improvements in the art of living that can be brought

* According to a more recent military order, the Navajo tribe has been returned to their original locality west of the Rio Grande, where they are making encouraging advances in civilization.

within the scope of their capacity, having an orderly system of government, regulating all their social and business relations, indulging only in defensive warfare, and in every respect exhibiting the most encouraging features of aboriginal civilization.

The Pueblo Indians select, by preference, the larger valleys and fertile districts adapted to cultivation. Their towns are located with a view to defence and observation, and in the interior districts occupy those castellated rocks, corresponding in location and structure to the closely allied remains of the ancient civilization, of which, it is but reasonable to infer, this is but a remote trace.

In this natural parcelling out of a country vast in extent, favored by a most genial climate, and possessing great undeveloped natural resources, we have the means of estimating the character and prospects of an American civilization when transplanted and matured on this elevated central region—"this highway of the nations." Sheltered by high mountain ranges from the sweep of the freezing northern blasts, it presents an open, unobstructed slope to the moist southerly currents, borne respectively from the Mexican and the Californian Gulfs. Hence on the higher districts there is sufficient moisture precipitated to favor the growth of forests, and give origin to perennial streams, which supply the upper sources of the Rio Grande and the Gila. In these elevated valleys the general aspect is that of a northern region, characterized by spring and summer rains, light winter snows, and an elastic, bracing atmosphere. Extensive pine forests cover the higher mountain slopes, interspersed with well watered, grassy valleys, affording a most magnificent summer pasturage. All these manifest advantages are connected with a central geographical position, accessible by ordinary means of travel, and closely adjoining semi-tropical districts, lying to the south. By the navigable waters of the Colorado of the West it has direct water communication with the Californian Gulf and the Pacific Ocean, thus presenting a combination of desirable features that must eventually insure its civilized advancement.

Mineral wealth is an element that does not enter at all into the estimate of productive capacity as derived from a view of its aboriginal population, and this is one of the main qualifying considerations that must modify our views of a progressive civilization, planting a large population on otherwise forbidding districts. Thus, on the desert regions of Nevada, where a short time since digger Indians eked out a miserable existence on roots, pinons, lizards and grasshoppers, we have seen the development of a single rich mineral vein securing for such a locality a populous district, gathering from abroad all the desirable resources and luxuries pertaining to the most favored localities, in spite of all natural disadvantages. Much more, when such stimulating attractions as mineral wealth draws in its train are added to agreeable natural accessories, may we expect the influx of a prosperous and permanent population, and such will eventually be realized on the now undeveloped gold and silver lodes of New Mexico and Arizona.

The pioneer miner, artisan and agriculturist, are already there, spying out the land, maintaining an exterminating warfare with the relentless and untamable Apache ; along the travelled roads and obscure trails, every thicket and rocky covert has echoed with the crack of the hunter's rifle or revolver, and many a canon and rocky slope has been richly watered by pioneer blood. Out of this contest there can be but one solution —the wild Indian must give place to the progressive white man, Anglo Saxon civilization must supplant barbarism, and what the country is capable of being made under the energizing influence of American enterprise it will eventually become.

The experience of the last quarter of a century has abundantly shown that the natural tendency, alike of the popular and scientific mind, is to depreciate and underrate the actual value and natural resources of imperfectly known districts. It is within this period of time that it has been scientifically assumed that beyond the one hundredth degree of west longitude agriculture could not be successfully prosecuted, from the dimin ished quantity of atmospheric precipitation. We have now a practical refutation of this argument in the self sustaining agricultural communities of Colorado, Utah and New Mexico. Only within the last two or three years has it been successfully demonstrated that cereal products of superior quality can be matured, without irrigation, over extensive districts in New Mexico, Arizona and California. . In fact, wherever weeds grow along the road side, there agricultural products, adapted to the particular locality, can be profitably raised. Among the Pueblo Indians of New Mexico irrigation has always been but partially relied on for maturing crops, and with this important accessory carried forward on the enlarged scale of civilized appliances, extensive districts may be redeemed from sterility, and a security given to the returns of agriculture, such as does not pertain to those favored districts that depend solely on the varying contingencies of a regular rainy season. One very manifest advantage of irrigation, as an agricultural process, is realized in the entire security of the crop through the harvesting season, whence results that superior quality of grain, peculiarly adapting it for distant transportation. It is not intended by this strain of remark to assert that there are not in the region under especial consideration extensive desert tracts, and agricultural wastes unsuited for human habitation, but it is safe to say that the extent of these has been grossly exaggerated, and that they are so interspersed with fertile valleys, pastoral uplands, and a variety of natural products, both vegetable and mineral, as to compare most favorably with those Eastern districts whose natural resources are limited, and which are indebted to the old appliances of civilization for most of those advantages which render their particular localities adapted to the requirements of human habitation.

Let the facilities of railroads be once extended to the attractive regions of New Mexico and Arizona, and we shall not be long in witnessing the successful development of vast mining enterprises, the growth of cities, and manufacturing centres, the extension of agriculture over virgin wastes, unexhausted by culture; surplus commercial products, unrivalled in extent, or cheapness, and with the disappearance of the nomadic Indian, we shall soon realize the influx of a permanent population before which the highest attainments of the ancient civilization will hardly afford the means of a faint comparison.

6. On the Leaves of Coniferæ. By Thomas Meehan, of Germantown, Pennsylvania.

Botanists can scarcely have overlooked the fact that the true leaves of *Pinus* consist of bud scales; and that what are known as leaves, and what Dr. Engelman (Gray's Manual, fifth edition, p. 469) calls "secondary leaves," are but phylloid shoots, but I have failed to find any specific reference to the fact in botanical works. Dr. Dickson, however, in a paper on the *Phylloid Shoots of Sciadopitys verticillata* ("Proceedings of Botanical Congress," 1866, p. 124) remarks, "In *Sciadopitys* I have to call attention to the fact that the leaves of the growing shoots consist, as in *Pinus*, entirely of bud scales." One would suppose from this incidental reference to *Pinus* that he was acquainted with the fact that the so-called leaves of *Pinus* were phylloid shoots; but as the object of the paper is to show that the so-called leaves of *Sciadopitys* are not true leaves, and as any one must know that they are not if already cognizant of the fact in *Pinus*, we may take it for granted that at any rate if not entirely overlooked, little thought has been given it. I believe I am occupying an entirely original field in pointing out the true nature of leaves in *Coniferæ*, and that the increased knowledge will have an important bearing on many obscure points in their study.

Dr. Dickson uses but the language of general botany when he describes the true leaves of *Pinus* as "bud scales," meaning thereby the scaly free portion just under the "secondary leaves," of Engelman, and sometimes forming sheaths around them. But these free scales are scarcely leaves. The chief portion of the true leaves in most plants of the order are adnate with the stem. Sometimes they have the power to develop into scaly points, at others into foliaceous tips, and at other times without any power but to preserve their true leaf-like character. *Larix* affords the best illustration. The true leaves are linear spathulate, entirely adnate to the stem. There are two kinds of stem growth on *Larix*. In the one case the axis elongates and forms shoots; in the other axial development is arrested and spurs are formed. On the elongated shoots the leaves are scattered; on the spurs they are arranged in whórls. The power of elongation possessed by the shoot is imparted to the leaves which are adherent to it, and they produce green foliaceous awl-like tips; the power of elongation which the spurs have lost is also measurably lost to their leaves, they develop themselves fully, although they have no stem to adhere to; they preserve their spathulate form, but cannot produce the awl-shaped tips of the shoot leaves. There are, therefore, two forms of leaves on the larch, the one free, the other adherent; and we have a novel principle very clearly illustrated that *strong axial development* (vigor) *is a characteristic of adhesion, while the reverse* (weakness) *is characterized by a free system of foliation.* Any species of *Larix* will sustain this observation; and, *leptolepis*, as a vigorous grower, is the best.

The characteristics of the foliage described in *Larix* may be found in a greater or less degree in a great many species of coniferous plants. In *Cryptomeria* the leaves adhere for four-fifths of their length on vigorous shoots; but on the more delicate ones they are free for three-fourths or more. In *Juniperus* the different forms of foliage are well known, especially in *J. Virginiana*, *J. Chinensis*, and *J. communis*. On the vigorous shoots adhesion takes place for nearly the full length of the leaves, but on weaker ones the leaves are very nearly free. In *Thuja*, *Biota*, *Retinispora*, *Cupressus*, *Thujopsis*, indeed most of the Section *Cupressineæ* these variable degrees of adhesion may be found, and always in relation to the absence or presence of vigor; and on this question of vigor it will be well here to make a few remarks. The power to branch I take to be a high mark of vigor. The young seedlings of most coniferous trees grow but a few inches the first year and have no power to branch. The power increases with age, and in all cases in proportion to the vigor of the plants. In *Thuja*, for instance, no branches appear till the second year. They increase in number, until, when in its prime, branches appear from every alternate pair of axils, and, as these are decussate, this gives the fan-like form of growth of which the Arbor Vitæ affords a familiar illustration.

This varying power of adhesion in the true leaves, and in connection with vigor enables us to explain many matters hitherto not understood. For instance, Dr. Lindley describes a form of *Biota* as *B. Meldensis*, suggesting that from its appearance it must be a *hybrid between the Red Cedar and Chinese Arbor Vitæ.* It is but *B. orientalis* with the leaves moderately united. *Thuja ericoides* of gardens was long supposed to be a Japanese species; it is but an entirely free-leaved form of *Thuja occidentalis. Retinispora ericoides* of Zuccarini is but a free-leaved form of some Japanese plant; and in all probability many species of *Retinispora* so marked in herbariums, are all forms of one thing with more or less adnated leaves. In all these cases delicacy of growth and freedom of leaves go gradually together as before indicated.

One of the most remarkable instances of the value of this principle however, will, I have no doubt, be in fixing the identity of the Japanese genus *Glyptostrobus* * of Endlicher, with the American *Taxodium* of Richard. In a shoot one foot in length of the latter we find perhaps four or six branchlets; in the same space in *Glyptostrobus* we shall find a score or more. Indeed, in this plant, a branchlet springs from nearly every axil on the main branch, showing an extraordinary vigor. As vigor is opposed to a free development of foliage, the small thread-like leaves of *Glyptostrobus* are naturally to be expected, and the free leaves distichously arranged is the natural concomitant of the weaker *Taxodium.* Fortunately I am able to sustain this theory by actual facts. I have a seedling tree ten years old of remarkable vigor. It does not branch quite as much as the typical *Glyptostrobus*, but much more freely than any *Taxodium.* The result is the foliage is aciculate, not distichous, and just intermediate between the two supposed genera. But to help me still more, my tree of *Glyptostrobus* has pushed forth some weak shoots with foliage identical in every respect with the intermediate *Taxodium.* Specimens of all these are presented with this. In establishing *Glyptostrobus*, Endlicher notes some trifling differences in the Scales of the Cones between it and *Taxodium*, but all familiar with numerous individuals of some species of *Coniferæ*, *Biota orientalis* for instance, know how these vary. There can be no doubt I think,

* *Note by the proof reader,*—It was the intention of the author to refer his remarks on Glyptostrobus to *G. Sinensis* Endl.

of the identity of the two; and this will form another very interesting link in the chain of evidence, that the flora of Japan is closely allied to that of the United States.

If we were to look on the so-called leaves of *Pinus* and *Sciadopitys* as true leaves, we should find serious opposition to my theory that a vigorous axial growth is opposed to the development of free leaves in *Coniferœ*, for we should see a class of plants which notoriously adds but from three to six branches annually to each axis, clothed with foliage. But admitting them to be phylloid shoots, it confirms our theory in a strong degree. We then see a plant loaded with branchlets, and so great is the tendency to use them instead of leaves, that in some cases as in *Pinus strobus*, *P. excelsa*, and others of a softer class of *Phylloideœ*, the bud scales are almost entirely confined to the sheathing leaflets; just as in the very rugged, hard leaved, almost spinescent forms, like *Pinus Austriaca*, we find them more dependent on well developed adnate leaf scales. In *Abies* of old authors, *A. excelsa* for instance, we have a numerous branching tendency; hence we have true leaves though partially adnate, and no necessity for phylloid branchlets. In *Picea* of Link, almost near *Abies*, taking *P. Balsamea* as a type, we have a rather weaker development, slower growing and less hardy trees, and the leaves are nearly free. Could some of the shoots of *Abies* be arrested in their axial development as in *Larix*, we should have the remainder increased in length, and the fewer branchlets, and two forms of leaves just as in *Larix*. Should, on the other hand, the plant increase in vigor there would be no class of free leaves, adnation would be the law, and metamorphosed branchlets prevail. Starting from *Abies* extra vigor makes the Pine; extra delicacy the Larch. It is the centre of two extremes.

That the fascicles in *Pinus* are phylloid shoots I think cannot be questioned. Their position in the axils of the true leaves, as beautifully shown in *Pinus Austriaca*, indicates the probability. Their permanency in proportion to their induracy is also another strong point. The soft ones of the Strobus Section retain vitality little longer than some true leaves, while the spinescent ones of *P. Austrica* remain green for four or five years. But the strongest of all points is that on dissection of an old fascicle it will be found to have a distinct connection with the woody system of the tree, and that these minute woody axillæ under each fascicle, increase in size with the age of the sheath. With a very little encouragement these woody axillæ can be induced to elongate and become real branchlets. If the leading shoot, for instance, of a Pine be tipped in May just after pushing, bulblets will form in every fascicle below, and the next season, the bulblets will produce weak branchlets, although this might be said of true leaves, which are supposed to bear an embryo shoot in the axil of every one. So much stress need not be put on this fact as the others are sufficient. It is introduced and its weak nature commented on, as it furnishes the chief point in Dr. Dickson's argument for *Sciadopitys*, which amounts to little more than that the apparently single Phylla is really a double one—a two-leaved fascicle united by a transformed sheath through its whole length. Carriere has since pushed Dr. Dickson's observations farther by noting in the "Reveu Horticole" really bifid leaves, with little verticils in the axils (see reference in "Gardener's Chronicle," May 2, 1868), an observation which I confirm by a specimen exhibited herewith; yet as I have said it is by itself not wholly free from the objection that it may be but a modified form of regular bud growth; but together with my other observations I think they do serve to confirm the point of these so called leaves being but phylloidæ.

In conclusion I will restate the main points of this paper:

The true leaves of Coniferæ are usually adnate with the branches.

Adnation is in proportion to vigor in the genus, species, or in the individuals of the same species, or branches of the same individual.

Many so called distinct species of coniferæ are the same, but in various states of adnation.

7. DESCRIPTION OF A NEW SPECIES OF PROTICHNITES, FROM THE POTSDAM SANDSTONE OF NEW YORK. By Professor O. C. Marsh, of New Haven, Conn.

THE first discovery of footprints in the lower Silurian of this country, appears to have been made in 1847, in the Potsdam sandstone of Beauharnois, Canada East. In 1851 an account of the locality was published by Sir William E. Logan, and with this appeared a short description of the impressions themselves by Professor Owen, of London, who then considered them the tracks of a Tortoise.* Subsequent explorations by the Geological Survey of Canada brought to light new localities and additional specimens in the same region, and in the following year the geological age of the strata containing them was fully discussed by Sir W. E. Logan, and a more detailed description of the footprints was given by Professor Owen, who now regarded them as made by a large Crustacean, probably allied to the modern *Limulus*.† He applied to these impressions the generic name *Protichnites*, and distinguished six species, or varieties, apparently quite distinct from each other. A trail of a different character was afterwards discovered in the same formation near Perth, Canada West, and described by Sir W. E. Logan under the name *Climactichnites Wilsoni*.‡ This was supposed to be the track of a Gasteropod, but Professor Dana has since suggested that it may have been made by a large Trilobite.§ Up to the present time, eight localities of footprints have been found in the Potsdam sandstone of Canada, along the strike of the formation, for about four hundred miles, and all at nearly the same horizon, or within fifty to seventy feet of the top.‖

From the lower Silurian of the United States, no footprints appear to have been described hitherto, although their existence in these strata is now clearly established. An interesting series of impressions was discovered by the author on the western shore of Lake Champlain during a visit to that region

* Journal Geological Society of London, vii, pp. 247 and 250.
† Journal Geological Society of London, viii, pp. 199 and 214.
‡ Canadian Naturalist, v, p. 279, 1860.
§ Manual of Geology, p. 189.
‖ Geology of Canada, p. 108, 1863.

immediately after the meeting of the Association at Burlington in August last. They were found on a ripple-marked surface of Potsdam sandstone, just above the then water-line of the lake, a short distance north of the village of Port Kent. The rock was a hard white quartzite, resembling that containing the footprints in Canada, although probably belonging to a somewhat lower horizon. The impressions obtained, which indicate a new form of Crustacean track, were in two portions on the same surface, and were evidently made by the same animal. They form together a series of footprints about six feet in length, consisting of two parallel rows of impressions, separated from each other by a space of about one and three-fourths inches, and having an extreme width between their outer edges of two and one half inches.

One of the most striking features of this series, which readily distinguishes it from the *Protichnites* already described, is the absence of a medial trail or tail-mark. No indications of such an impression could be detected, even when the track passes over the ridges of the ripple-marks, where it certainly would appear, if at all. The footprints indicate, moreover, a much smaller animal than those that made the other forms of *Protichnites*, the width of the series being in general less than one-half that of the other species, excepting *P. multinotalus*, from which, however, it differs in the absence of the medial groove, and in the less numerous and more irregular foot-marks.

The margins of the individual impressions are not, in general, accurately defined, partly, perhaps, owing to the fact that the surface of the sandstone had been somewhat worn by the water and ice of the lake. In this way some of the smaller impressions may have been obscured or obliterated, as it is now difficult to trace with certainty, through the whole series, the successive groups of footprints which correspond to the repeated applications of the same limbs of the animal, although such a repetition is easily recognized where the tracks are best preserved. In such places the impressions are seen to be arranged in groups of six, which in each row evidently represent successive applications of the same series of limbs, the corresponding set on the other side making a group nearly or quite opposite when the animal moved directly forward, but partially alternating when it turned to the right or left. Each group appears to consist of four impressions in the linear series, and two additional ones inside and a little behind the last two. The divergence of the impressions thus produced probably indicates, as Professor Owen has shown in regard to the other species, the direction in which the animal was moving. The bottom of some of the tracks are sharply defined, and were evidently impressed by some hard pointed limb. This would tend to show that they were not made by a Trilobite, to which their origin might otherwise naturally be attributed, while the absence of a medial groove would probably exclude a Crustacean of the type that made the other *Protichnites*. The points of difference between this form and those hitherto described are sufficiently marked to justify its separation, and it may appropriately be named *Protichnites Logananus*, in honor of Sir W. E. Logan, Director of the Canadian Geological Survey, to whom this department of palæontology is so much indebted.

The peculiar interest attached to these various footprints on the oldest Silurian strata depends in part upon the fact that we thus have evidence of forms of life before unsuspected; since no other indications of the animals that made the impressions have as yet been discovered. An exception should perhaps be made of the small Limuloid Crustacean (*Aglaspis Barrandi*) described by Professor Hall, from the Potsdam sandstone of Wisconsin and Minnesota, which may have made footprints somewhat similar to the *Protichnites*.* Indistinct impressions, resembling those from Canada have indeed been observed by Mr. Daniels in that formation in Wisconsin, although not at the same horizon in which the remains were found. The *Aglaspis* of the Potsdam and the *Eurypterus* of the Upper Silurian at least indicate the general affinities of the Crustaceans that have recorded their existence in the *Protichnites*, and the remains of the animals themselves will, doubtless, be brought to light at no distant day, and thus reveal their true nature.

* Sixteenth Regents Report on N. Y. Cabinet, p. 181, 1863.

8. On the Preservation of Color in Fossils from Palæozoic Formations. By Professor O. C. Marsh, of New Haven, Conn.

(ABSTRACT.)

The fossil shells in the more recent formations, especially those of Europe, have not unfrequently been found to retain their original color-markings more or less perfectly; but specimens in this state of preservation have very rarely been detected hitherto in the older rocks. The few instances observed, however, are of especial interest to geologists, since the investigations of Professor E. Forbes and others have shown that, in the existing seas which have been explored, shells with well defined color patterns are in general only found in comparatively shallow waters, and the same species may be highly colored when near the shore, and white when inhabiting depths below one hundred fathoms. This was probably true also of the Mollusca in the palæozoic seas, and we may thus have some indication of the depths in which they lived.

The first specimen with the color distinctly preserved, which appears to have been noticed in European palæozoic strata, was a *Pleurotomaria*, from the subcarboniferous limestone of England, described in 1836 by Professor Phillips as *P. flammigera.** Subsequently one or two other species from the same formation in Ireland were found to be similarly marked, especially the common *Terebratula hastata* Sowerby, and from this fact Professor Forbes inferred that the limestone containing them was deposited in an ocean not exceeding fifty fathoms in depth—a conclusion, however, somewhat at variance with the results of more recent explorations.†

Probably the most interesting fossil, with color markings, from the palæozoic yet discovered is a specimen of *Orthoceras*, found in the Devonian at Paffrath on the Rhine, and described in 1842 by M. d'Archiac and de Verneuil as *Orthoceratites anguliferus.*‡ This specimen, which is now in the private collection of M. de Verneuil in Paris, is still covered with perfectly

* Geology of Yorkshire. Part II, p. 226.

† See paper by M. Sars. Videnkabs-Selskabs Forhandlinger, pp. 246–275, 1868; and Annals and Magazine of Natural History, p. 423, 1869. Also Count Pourtale's Contributions in Bulletin of Museum of Comp. Zöology, p. 126, 1868.

‡ Transactions of the Geological Society of London, 2 series, vol. i, p. 346. 1842.

defined zigzag bands of the original color. While in Germany in 1865, the author was fortunate enough to procure from the same locality a second specimen of this species, with similar bands of color, and these two are probably the only specimens yet discovered. Several gasteropods (*Natica subcostata* Schl.), with distinct color markings, were obtained at this locality at the same time, and others have occasionally been found there.

These discoveries in European strata led the author to examine the palæozoic fossils of this country, where the subject appears to have attracted but little attention as yet, and he had already found indications of color in specimens from several formations. The most interesting of these were Cephalopods from the Trenton limestone of New York, especially *Endoceras proteiforme* Hall, which in several instances he had found to retain distinct traces of the original color, arranged in delicate cancellated patterns. In other species from this formation, and particularly in some *Orthocerata* from the Hudson and Niagara limestones of Illinois and Iowa, he had also noticed indications of color markings. The author exhibited a series of fossils from the palæozoic rocks of Europe and this country, all showing indications of the original color, and stated that a careful examination of our palæozoic strata, especially the light-colored limestones and shales of the West, would undoubtedly bring to light many additional specimens, and thus afford new evidence of the condition of the seas in which they were deposited.

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9. Announcement of the Existence of Cretaceous Rocks in Guthrie County, Iowa. By C. A. White, M. D., of Iowa City.

In July, 1867, I announced in the American Journal of Science and Arts, the discovery of the sandstones of the Dakota group of Cretaceous rocks as far south in Iowa as Red Oak Junction, Montgomery county, and within thirty miles of the northern line of the State of Missouri.

I have now to announce the discovery of the same rock in Guthrie county; one locality being within less than forty miles of the city of Des Moines, and more than eighty miles eastward from the Missouri River.

Further investigations concerning its distribution are now in progress, which it is believed will result in showing a much greater easterly extension of that formation than it has been hitherto believed to possess.

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10. On the Geology of Lower Louisiana and the Rock Salt Deposit of Petite Anse. By Eugene W. Hilgard, Ph. D., of Oxford, Mississippi.

(ABSTRACT.)

The discovery in 1862, of a deposit of rock-salt on the coast of Louisiana, was a fact so unexpected to geologists, that at any other time a detailed investigation of its geological relations would quickly have followed the first announcement. The pressing necessities of the blockaded section soon caused its development on a large scale, though in a very irregular manner; for a considerable period, these mines supplied the whole of the south-west. In November, 1865, Professor Richard Owen made a brief examination of the locality, the results of which he published in the Transactions of the St. Louis Academy. A year later Dr. Charles A. Gössmann, under the auspices of the American Bureau of Mines, made an examination of the locality, mainly with a view to the working of the deposit. His report, published by the Bureau, as well as the specimens which he courteously exhibited to me, confirm previous conjectures that the overlying strata were the equivalents of the formation I have described as the "Orange Sand" of Mississippi. I therefore gladly availed myself, at the earliest possible moment, of the offer of the Smithsonian Institution to defray my expenses in making a detailed geological investigation of the region. The low stage of water prevailing at the time (December, 1867) rendered it possible to observe, to the best advantage, the formations exhibited on the banks of the Mississippi; the examination of which, from Vicksburg to the Passes, was a needful preliminary step to the determination of the formations of the coast.

Having previously examined and described the sections exhibited at Vicksburg, Grand Gulf and Fort Adams,* I merely landed at some intermediate points to verify the conclusion previously reached, viz., that below Vicksburg no marine formation crops out on the river banks, reports to the contrary notwithstanding; and that the profiles at Natchez, Rodney and other points are essentially similar to that at Fort Adams, where we find the strata of the (fresh-water) "Grand Gulf group" in a position nearly or quite horizontal; overlaid, first by the materials of the "Orange Sand," which in its turn is capped by the stratum of the "Loess" or Bluff formation, covered by a thin deposit of "Yellow Loam."

Facing southward from the "Blockhouse hill" at Fort Adams, we observe a wilderness of the characteristic sharp ridges of the Loess region, often fore-shortened into veritable peaks, elevated between 300 and 400 feet above the river. In this region the Grand Gulf strata have been traced southward by Dr. George Little, the present State geologist of Mississippi, as far as the head-waters of Thompson's Creek, northwest of Clinton, La.

The Orange Sand proper is visible, near the river, as far south as Jackson, La., but farther inland extends to a lower latitude. As for the Loess, it appears in full force and characteristically developed for some distance south of Fort Adams. But (according to Dr. Little's observations) these features become gradually modified as we advance southward. The Loess deposit thins out, its materials become poorer in lime and fossils, and assume more and more the character of a common fine grained "hardpan"; the transition being by insensible degrees, while the two extremes are very obviously distinct. At the same time the clayey substrata which, farther above appear only in patches (as at Nevitt's bluff, two miles above Natchez, as well as at the latter place itself) are seen more frequently and continuously, until, at Port Hudson, they become predominant.

The exposure at Port Hudson, previously examined in part by Bartram, Carpenter and Lyell, is about three miles in extent, from the mouth of Sandy Creek above the town, to Fon-

* See Report on the Geology and Agriculture of Miss., 1860.

tania Landing 1½ miles below. Its lower half is washed, and continually encroached upon, by the river; its upper portion is now inland of an extensive sand-bar. The strata are disposed horizontally or basin fashion, and vary a good deal both in thickness and materials, as shown in the subjoined profiles, situated about a mile apart; the correspondence of strata is ascertained by actual tracing of the stratification lines.

Near Sawmill, Port Hudson.	No.	*Midway betw'n Port Hudson & Fontania.*
Yellow surface loam, 4—6 ft.	6	Yellow loam, sandy below; 8—10 ft.
Yellow hardpan, ... 25 ft.	5	White and yellow hardpan, ... 18 ft. . . . . Orange and yellow sand, sometimes ferruginous sandstone, irregularly stratified, ... 8—15 ft.
Heavy greenish clay, ... 7 ft.	4	Heavy, greenish or bluish clay, . . 7 ft.
Gravel, sand and clay in irregular bands, like river alluvium; with pebbles, driftwood, leaves, and Mastodon bones, ... 6 ft.	3	White indurate silt or hardpan, 18 ft.
Heavy, greenish or bluish, massy clay, similar to No. 4; 25 ft. visible.	2	Heavy green clay, with porous calcareous concretions above, ferruginous ones below; some sticks and impressions of leaves, ... 30 ft.
	1	Brown muck, ... White or blue clay, } with cypress stumps, 3—4 ft.

At the stage of extreme low water prevailing at the time, the stump stratum No. 1 was visible to the thickness of 10 feet at its highest point; showing several generations of stumps above one another, also the remnants of many successive falls of leaves and overflows. The wood is in a good state of preservation; no prostrate trunks to be seen at present.

The main clay deposit, No. 2, varies but little in general character; although very solid, its tendency to cleave into prismatic forms renders it very liable to "cave" into the river. The upper portion of the stratum, especially near its southern end, contains strings of calcareous nodules, on stratification lines eight to twelve inches apart. No fossils save rare impressions of leaves.

No. 3 is exceedingly variable. At the northern end of the outcrop, it is a narrow band of swamp deposit; at the first of the profiles given, it bears the character of a sand-bar; lower down it returns to that of a swamp deposit; still below it is represented by a fine white silt, without a trace of vegetable remains. Lower down again a lignitic layer appears at its base, with leaves and fruit of living species of lowland trees; while near Fontania it is again a sand-bar with an abundance of prostrate trunks of drift-wood, coarse sand and pebbles.

The green clay stratum No. 4 varies little, either in thickness or composition, and like the stump stratum, No. 1, forms a convenient level of reference.

The hardpan stratum No. 5 I conceive to be the more immediate representative of the Loess proper, with which it is connected by gradual transition, though at times greatly resembling some of the materials of the Orange Sand. It is void of fossils.

The present profile differs in many respects from those given by previous observers, which lay some distance farther west, where the river now flows. The strata are accordingly as variable in an east and west as in a north and south direction, and with the exception of Nos. 1 and 2, are such as are now shown in ditches cut into the modern river-bottom deposits.

The stump stratum, No. 1, however, as appears from numerous data collected by myself or contained in Humphrey's and Abbot's Report on the Mississippi River, exists at about the same level (*i.e.*, near that of tide-water), not only over all the so called Delta-plain of the Mississippi, but also higher up, perhaps as far as Memphis, and all along the gulf coast, at least from Mobile on the east to the Sabine River. Wherever circumstances allow, the overlying clay stratum, No. 2, is also observed. These facts indicate the wide spread prevalence, during the epoch succeeding the drift, of quiet, shallow freshwater lagoons and swamps of slightly varying elevation; through which the continental waters may for some time have found an outlet, without a definite channel, representing the Mississippi of to-day. The Port Hudson profile appears to be typical, its features being reproduced wherever denudation has not removed these deposits down to the level of the stump stratum, as is mostly the case.

THE FIVE ISLANDS.

The chain of five islands rising partly from the sea, partly from the coast marsh, between the mouth of the Atchafalaya and Vermilion River, have been described by Mr. Thomassy,*

* Géologie pratique de la Louisiane; New Orleans, 1860.

who attributes their origin to "hydrothermal" or "volcanic" action. His descriptions are sufficiently faithful to show the general resemblance of their geological structure; so that after visiting the three middle members of the chain, viz.: Cote Blanche, Week's Island, and Petite Anse, I have thought it superfluous to extend my examination to the two extreme ones, viz.: Belle Isle, the promontory west of Atchafalaya Bay, and Miller's Island (or "Orange Grove") overlooking the plains of the Vermilion. These elevations lie nearly in a straight line bearing N. W. by W. from Belle Island.

COTE BLANCHE.

The next in order, affords on its sea-face a fine exposure of the lower members of the Port Hudson profile. At tide-level we have the blue clay with cypress stumps, the tops of which are often surrounded by alternate layers of clay, muck, and sometimes lignite. The overlying strata consist partly of blue clay similar to No. 2 at Port Hudson, partly of various colored loams alternating with the former; and exhibiting the same calcareous or ferrugino-calcareous concretions along the stratification lines. At a few points these calcareous concretions resolve themselves into distinct fossils, representing the fresh-water genera *Paludina*, *Melania*, *Unio*, *Anodonta* and *Cyclas*, in an indifferent state of preservation. The entire visible profile is about fifty feet high; the highest point of the island rises as high as 180 feet, but in its interior no exposures exist, so that the higher members of the series are not verifiable.

WEEK'S ISLAND.

This island, lying 6 miles N. W. by W. from Cote Blanche, has an area slightly greater, viz., 2,300 acres; it is nearly circular; maximum elevation 160 feet above tide water. Unlike Cote Blanche it is traversed by deep ravines which exhibit the geological structure. In the central and highest portion these gullies are bordered by steep slopes composed of the most characteristic materials of the Orange Sand group. On the exterior slopes, however, we find in a position inclined away from the centre of the island, the lower strata of the Port

Hudson profile—green or blue clay with calcareous concretions, and imperfect fresh-water shells. The blue clay stratum with cypress stumps is met with in ditching, and is also known to exist in the beds of the neighboring bayous, as well as in the surrounding marsh.

PETITE ANSE, OR AVERY'S ISLAND.

Petite Anse lies about 12 miles N. W. by W. of Week's Island, and in its general structure much resembles the latter, to which it is slightly inferior in size, and about equal in elevation; its highest point, "Prospect Hill," on the north side, being 160 feet above tide-level.

An elevated ridge connects Prospect Hill with another high point near the southern slope of the island; and near the west end a ridge, on which Judge Avery's house stands, falls off steeply toward the Bayou Petite Anse. These three points inclose the valley in which the salt deposit has been found and which opens south-eastward into the marsh.

The topography of the island, as well as the history of the mine, have been ably given by Dr. Charles A. Gössmann of Syracuse, in a report of the American Bureau of Mines.* Up to the time of his visit all the pits and shafts had been sunk through detrital strata, washed down from the adjoining hills, and frequently inclosing the vestiges of both animal and human visits to the spot. Mastodon, buffalo and other bones; Indian hatchets, arrowheads and rush baskets, but above all an astonishing quantity of pottery fragments, have been extracted from the pits. The pots doubtless subserved the purpose of salt-boiling; human handiwork has, however, been found so close to the surface of the salt, as to render it likely that its existence in mass was once known, before the time when, in 1862, Mr. D. H. Avery struck the salt itself at the bottom of a salt water well.

The surface of the salt undulates considerably, so that borings commenced at different levels have repeatedly struck salt at nearly the same relative depth, the absolute level of the rock-salt surface varying from 32 feet below to 1¾ feet above tide-level. The salt stratum has itself been penetrated to the

* On the rock-salt deposit of Petite Anse, New York, 1867.

depth of 38 feet, without any perceptible variation in quality; its "floor" being as yet unknown. Dr. Gössmann's observations and specimens proved to his and my satisfaction the existence of the Orange Sand on the island; but its relation to the rock-salt, and the age of the latter, remained undetermined.

Since then, another shaft has been sunk by Mr. Chouteau of St. Louis, with the assistance of Mr. Dudley Avery, to whom I am indebted for a record of the strata penetrated. This shaft was located at a higher level than any previously sunk, on a hillside where, not far off, the Orange Sand crops out *in situ*. After passing through these strata the rock-salt was struck again, at a level several feet higher than on any former occasion.

There can therefore be no doubt that the salt deposit is older than the Orange Sand, which here, as at Weeks's Island, forms the nucleus of the mass on whose outer slope, as well as its higher points, the strata of the Port Hudson profile reappear characteristically; with calcareous nodules, fresh-water shells and aquatic plants identical with living species. Not only is the reference level of the cypress stump stratum the same as elsewhere, but the green clay band, No. 4 of the Port Hudson profile, is also there.

The stratigraphical disposition of these deposits was quite remarkable. They conform not to the *strata*, but measurably to the *outline* of the Orange Sand nucleus, roughly following its slopes and curvatures. At first sight therefore it seems as though a local upheaval had taken place, and hence arose, probably, the reports attributing a volcanic origin to these elevations, whose isolated position in the level coast region would naturally give rise to speculation as to their mode of formation. Indeed the extent to which these strata are sometimes seen to dip rather staggers the observer; but the upheaval hypothesis does not explain the facts, unless we are content to assume a separate effort of the sort for every hillock on the islands.

There can be no doubt that subsidence subsequent to deposition has been the cause of the extravagant dips observed sometimes. Where the Port Hudson series is more immediately superimposed upon the Orange Sand nucleus the dips are moderate, and such as may well be assumed as resulting from deposition on inclined surfaces. But when we see an apparently undisturbed clay-stratum moving down hill like a glacier, so as to overthrow a deposit of loose stones, we need not go far to find the cause of extensive dislocation and subsidence.

BELLE ISLE AND MILLER'S ISLAND.

All the data I have been able to collect concerning the structure of these exterior islands tend to confirm the probable supposition that, like the three interior ones, they consist of denuded nuclei of Orange Sand materials, upon which the Port Hudson series was afterwards deposited.

It seems likely that the same is true of a low ridge called Cote Gelée, in Lafayette parish, bearing N. or N. N. E. Thomassy places in the same category the Grand Coteau des Opelousas and the Avoyelles prairie.

AGE OF THE SALT DEPOSIT.

The Orange Sand strata so rarely approach the coast that the deposits underlying them in the Coast region have scarcely been observed with certainty. Even the older strata underlying the blue stump clay have been observed at a few points only, viz.: by the Delta Survey in the bed of the Mississippi river at Bonnet Carré and Carrollton, near New Orleans; at the latter city itself, in the boring of wells; at Salt Point, on Bayou Sallé; and on the coast of Mississippi Sound.

The strata penetrated in the borings at New Orleans are considered by Sir Charles Lyell as Delta Deposits. But according to my examination they are almost throughout demonstrably of marine origin, and while the species they contain are mostly (not all) now known to be living on the Gulf coast yet the prevalence of species is very different from that now observed near the mouths of the Mississippi. In this respect the fauna of these strata shows a great analogy to those described as Pliocene by Tuomey and Holmes, occurring on the Carolina coast.

It is most probable that the rock-salt of Petite Anse will be found when pierced, to be imbedded in the equivalents of the deposits penetrated at New Orleans and Bayou Sallé, and of corresponding, probably early quaternary, age anterior to the drift or its southern representative, the Orange Sand.

ORIGIN AND EXTENT OF THE SALT DEPOSIT.

The absence of layers of the usual impurities of rock-salt, especially of gypsum, has induced Dr. Gössmann to suppose that it is not the result of the evaporation of sea-water, but owes its formation to crystallization from the purer brine of salt springs.

Our knowledge of the facts is still too limited to render a discussion of this point very profitable. In a very deep lagoon, withdrawn from the influx of the tides after the brine had acquired a considerable degree of concentration, all the gypsum might be found in a single bed at the bottom; upon it a large mass of pure salt, as in the present case; while the salts of the mother waters would naturally have been washed away from the top. Or there might have been a succession of lagoons communicating with each only during high tides, and acting in a manner analogous to the process now practiced in salt-making on the sea-shore. The gypsum would then all have been deposited in the outer lagoons, while the inner ones would have acted as brine-pits, where pure salt alone could crystallize. Crystals of gypsum have repeatedly been found in shallow wells on the coast beneath the "stump clay."

Upon any of the foregoing suppositions, calling into play a variety of circumstances not likely to be all simultaneously fulfilled, it does not seem probable that the rock-salt mass is very extensive horizontally, or that such masses should occur frequently in the coast region.

A mass of salt 144 acres in extent and 38 feet thick is, however, a handsome specimen, even if these dimensions should represent maxima. The great difficulty in mining it, heretofore, has been the influx of water through the gravelly strata overlying. But it has most probably been attacked, thus far, at its lowest surface level. Wherever elsewhere the Orange Sand formation prevails, it rests on a deeply denuded surface; and "hills within hills" are of very common occurrence. From the data thus far obtained it appears that the same is the case with the rock-salt mass, and that its surface roughly conforms to the hills and valleys now existing. Workings should be begun at higher levels; and it would not surprise me to learn that the auger had shown the mass to be accessible by level adits in lieu of shafts, on the hillsides. The interior of the solid mass once gained from a point secure from surface water, all difficulty would be at an end.

GEOLOGICAL HISTORY OF THE LOWER MISSISSIPPI VALLEY.

It appears from the facts stated in the preceding pages, that after the termination of the epoch of that Eocene period, represented by the Vicksburg group of fossils, down to the Quaternary era, marine deposits ceased to be formed on the northern border of the basin now represented by the Gulf of Mexico.

I have acquired the certainty of the existence over a large portion of northern Louisiana of the "Grand Gulf" series of rocks. From specimens in the collection of the New Orleans Academy of Sciences, it appears that, apart from the usual materials forming these beds in Mississippi, they assume in the Harrisonburg region the character of compact limestone, which in places is said to be fossiliferous, and would thus furnish the clue to the age of the Grand Gulf group for which I have vainly sought in Mississippi. The problem is one of great interest, as it involves the question whether or not the Mexican gulf has, within comparatively modern times, been disconnected from the Atlantic Ocean. The absence of the cauldron in which the Gulf Stream is concocted might have exerted climatic influences reaching beyond the American continent, and would explain many discrepancies between ancient and modern faunas on the shores of the Atlantic.

It appears that similar limestones, almost assuming the character of black marble, occur in St. Landry parish, near Opelousas. Whether the southern outline of the formation passes thence toward the Calcasieu region, where petroleum has been found, or whether it trends north-westward into the parishes of Sabine and Natchitoches, where limestone and sandstone ridges also exist, is a question still open. In the latter case, this outline would conform to the general shore lines of the great cretaceous and tertiary Mediterranean.

In Mississippi the Grand Gulf series is mostly overlaid by the Orange Sand, deposited on a deeply eroded surface, and bearing itself the evidence of its formation by fresh water in a state of violent flow.* The southern outline of the main body of the Orange Sand runs southward of Opelousas, toward the mouth of the Sabine, whence, according to reliable information, a broad band of shingle extends toward Harrisonburg, Catahoula parish. This belt represents, probably, the most westerly bayou of the great Orange Sand Delta; while, as heretofore stated, the most easterly one extends from the neighborhood of Cairo along the western shore of the Tennessee river, down the valley of the Warrior toward the coast of Alabama. The middle and main pebble stream evidently follows in general the course of the Mississippi River; but leaving it at the point where that river suffers its remarkable deflection eastward, we find the remnants of its ancient "bar" in the chain of the "Five Islands," which lie directly across the shortest line by which the Mississippi could reach the Gulf, and no doubt have had their share in causing this deflection.

Both the size of the pebbles carried by this middle bayou, and their character proving transportation from high northern latitudes, show it to have been the main channel during the Orange Sand epoch. It is not surprising, therefore, that in the direction of its course the Orange Sand formation should extend farther south than anywhere else. The pebble-beds are now overlaid by fine sandy materials, proving a diminished velocity, owing, doubtless, to a general depression, but greater at the north than at the south.

While the lateral bayous descending through Louisiana and Alabama were closed at the end of the Orange Sand epoch, it is evident that the central channel continued open; inasmuch as the next succeeding deposit, viz.: the Loess lies in a trough-shaped depression of the Orange Sand materials, the line of contact being always conformable and devoid of any trace of atmospheric denudation. The perfect peroxidation of the materials of the Orange Sand would seem, nevertheless, to point to a certain period of exposure to atmospheric agencies,

* Am. Jour. Sci., May, 1866; Miss. Rep., 1860, p. 26 and following.

caused by a temporary diminution of the influx of northern waters, through the cessation of subsidence, perhaps.

During this epoch of quiet might have begun the formation of those extensive swamp and lagoon deposits, the lower members of the Port Hudson series, whose floor stratum with its superimposed generations of cypress stumps, indicate a slow secular subsidence. The velocity of the latter seems gradually to have increased until the growth of old trees became impossible, and finally, in stratum No. 3 of the Port Hudson profile, we again meet the evidences of currents moving sand, pebbles, and drift-wood.

Then follows the Loess proper, a deposit utterly devoid, in Mississippi and Louisiana, of any evidences of fluviatile action — a uniform silt even in profiles of eighty feet, with scarcely a vestige of stratification, and none but terrestrial fossils.

The precise circumstances under which such a deposit could be formed, are perhaps a little obscure. There must have been such a depression of the whole country as to transform the immediate valley of the Mississippi, as far as Keokuk, as well as the valleys of the larger tributaries, into estuaries of the Gulf of Mexico, containing a mass of water too great to be sensibly affected by the variations now causing the annual overflows of those rivers (for otherwise the deposit must have shown lines of deposition), yet possessing a gentle flow above (since the materials of the bluff formation of Missouri and Indiana exhibit signs of fluviatile action); quite fresh in its upper portions (where fluviatile shells are found), but rendered unfit for the life of either a fresh or salt-water fauna by an admixture of sea-water, in its lower and almost stagnant portion, at tide-level; and deriving its vestiges of animal life only from the "offscourings" of the adjoining unsubmerged lands.

Sir Charles Lyell* inclines to consider the Loess as the product of "successive inundations of a great river," the absence of stratification from such deposits having, apparently, an analogue in the alluvial deposit of the Nile. But the case is far from being analogous; for the same phenomena are still observed in the modern deposits of the Nile, and are clearly attributable to the peculiarities of the hydrographic basin of

*Principles of Geology, 10th edition, p. 464.

that river; whereas, in the modern alluvium of the Mississippi, it is exceedingly difficult to find a uniform stratum two feet in thickness. The Nile mud is each year derived from the same rivers of Abyssinia, and equalized by intermixture and subsidence during at least 1 500 miles of its course. On the Mississippi, on the contrary, the deposits of different annual inundations are readily distinguished by the inhabitants for years afterward, according as the Illinois, the Missouri, Ohio, Arkansas or Red River happen to have furnished the main influx. The absence of any such differences from the Loess can only be explained on the assumption that the mass of water filling the channels was too great to be sensibly affected by such causes, the more so as the continental surface was sensibly diminished in consequence of a depression which, as far south as Fort Adams, cannot have been less than 400 feet, and on the coast not less than 200, but more probably the same as farther above.

The existence of the elevations on the Louisiana coast, above described, renders it necessary to assume that at the end of the period of depression — the "Champlain epoch" — the entire delta-plain (so called) west of the Mississippi was covered by the deposits of the Orange Sand and Port Hudson series to an equal height; and that during the succeeding "Terrace epoch" of elevation, the veritable Mississippi — *our* Mississippi — swept away these deposits in excavating its present valley. At first it might sweep over or through the pebble ridge, but would finally turn to the direction of least resistance, leaving the "Five Islands" high and dry.

It would thus seem that, unlike other large rivers of the world which have from the outset added to the land by bringing down the materials to form their alluvial plain, the Mississippi has first formed by denudation the plain which it was subsequently to cover with its alluvial deposits to a comparatively inconsiderable depth. The western and southern limits of this denuding action would seem to be marked by the Grand Coteau des Opelousas, the Cote Gelée and the Five Islands; and the materials swept away from this area doubtless contributed largely to form the foundations of the truly alluvial plain extending south and south-eastward of lakes Maurepas, Pontchartrain and Borgne.

It is obvious how futile must be all attempts to estimate the age of the Mississippi river in absolute measure, by a comparison of the advance of its present delta into the Gulf, with the distance of its mouth from the divergence of bayous Plaquemine or Manchac. When the broad flood of the Terrace epoch contracted into the present Mississippi, that stream emptied into a sea rendered shallow by the deposition, within a comparatively short period, of a huge amount of material. Within such a sea its channel would be likely to change about, somewhat like those of the great rivers of China. *Now*, it is advancing into the deep water of the Gulf of Mexico, but at a very different rate, and by a very different process from that of simple alluvion. But the questions pertaining to this portion of the subject, together with the results of my observations in the Delta proper, I propose to discuss at a future time.

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11. Observation on the Red Quartzite Boulders of Western Iowa, and their original ledges of Red Quartzite in Iowa, Dakota and Minnesota. By C. A. White, M. D., of Iowa City.

Profusely scattered among the drift material of western Iowa, are fragments and sometimes large masses of very hard red quartzite, so constant in its physical characters and appearance, and so different from any other variety of rock contained in the drift as to readily attract attention.

Judging from published descriptions of the rocks observed by various persons in eastern Dakota and south-western Minnesota, I ventured to suggest in an article published in the May number of the "American Journal of Science and Arts" for 1867, that the red quartzite boulders of the drift of western Iowa were derived from those rocks, while the associated granitic and other boulders had a still more northern origin. While continuing my observations to north-western Iowa during the present summer, it has been my good fortune to determine, in a great degree, the eastern limit of distribution of those quartzite boulders, and to confirm my opinion, then expressed, by tracing those boulders, step by step, to their original homes in the ledges of rock which occupy the region before referred to, and to find the granitic boulders resting upon those ledges where they had been arrested in their icy journey from still more northern homes, and almost within the very grooves they themselves had made upon the quartzite.

The first exposure of the quartzite *in situ* was found in the extreme north-east corner of Iowa, where its outcrop makes a slight fall in the Big Sioux River; the same exposure extending eastward about a quarter of a mile to the point where the surveyors have placed a post of the same rock to mark the south-west corner of Minnesota.

The rock is distinctly stratified and has a dip here of six or eight degrees to the northward. Northward from this point, particularly near the Big Sioux River, the same rock is frequently exposed. At Sioux Falls, about eight miles in a straight line from the last named locality, the rock has the finest exposure yet seen. Here by measurement and estimate of the dip, which is here four or five degrees to the *southward*, we found a thickness of about three hundred feet. Upon visiting the red pipestone quarry, thirty-five miles north-northeastward from Sioux Falls, we find the same rock enclosing the layer of red pipestone.

Dr. Hayden and others have shown that this rock occupies a large area in this region, but it seems that no one has heretofore proven that it was the original deposit of those red quartzite boulders so common in western Iowa and eastern Nebraska.

Very distinct glacial scratches were seen in many places upon the surface of this hard quartzite, the direction of which vary in different places from five to twenty-five degrees both east and west of south, indicating no doubt mere local shiftings of the glacial current.

At the red pipestone quarry a group of four or five immense granite boulders, known in Indian legends as the "Medicine Rocks," rest upon the surface of the quartzite which they and others have so smoothed that the Indians have long used the surface to trace hundreds of hieroglyphic figures upon, by pecking it with sharp pointed stones.

In another place the direction in which the glacier was moving is clearly indicated by the splintered condition of the flinty rock on the proximal or northern side of vertical cracks and fissures in the mass, while on the distal or southern side the angle formed by the vertical and upper surfaces was always found complete. This effect could not be produced with the same distinctness upon a softer or less flinty rock.

No opinion as to the geological age of these rocks is yet matured, but they are known to be older than the Inoceramus beds of Cretaceous age. After dilligent search in every locality visited, as well as in hundreds of the glacier-moved masses, I have failed to find any trace or resemblance of any fossil whatever.

It is worthy of remark, however, that it is metamorphism alone that makes the quartzite lithologically different from the sandstones and ferruginous conglomerates of the Dakota group as seen in Iowa and elsewhere, while chemically they are in no essential degree different.

Those sandstones contain an occasional thin bed of clay, such no doubt, as the pipestone originally was, and the same metamorphic action that converted the sandstone into a quartzite, doubtless also made the clay a pipestone.

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12. On the Old Lake Beds of the Prairie Region. By Samuel Jacob Wallace, Keokuk, Iowa, 1868.

The prairie region of central North America, is not a mere surface formed from sets of level continuous strata eroded into such forms as drainage will produce.

They are spread over series of great swells of elevation and intermediate or enclosed basins, with river systems marking their lowest lines of flexion, and worn outlets from the basins.

These general basins contain the coal field and other deposits formed before the wearing down of the river ways.

Besides the system of basins forming the declines from the general elevations, slopes and swells, on which the surface and common strata are spread, there is another type of depressions, of more limited and definite boundaries, but less explicable origin.

These are usually referred to water-way erosions, without satisfactory explanation. Their forms are often extensions of greater or less length, from five to twelve miles wide, with remarkably direct, almost parallel and corresponding bluff side walls, lying at or near the low lines between great slopes of drainage. They are abrupt depressions of perhaps one to two hundred feet, by the discontinuance of solid strata and drift. Their floors are often flat lacustrine beds; in places, sand. Many have evidently formed lakes before the cutting down of the outlets, subsequent to the drift, and elevation of the surrounding country. Their form and position causes them to be occupied by general or local drainage ways; but without evident correspondence between their space and the general importance of the streams traversing them, which vary from almost nothing, up to the largest rivers.

Two principal instances of these depressions will be noticed. Both lie with their lines of extention near a north and south course. They form great curves to opposite sides. There is no apparent connection or system between them. The Mississippi River flows through one and partly through the other. Their north ends are both against east and west bluffs.

On the northern head bluff of one is the city of Muscatine, Iowa. After a length of fifty miles this depression abruptly abuts on another east and west bluff, at Dallas, Illinois.

Six or eight miles to the south-west from the south end of the first is a short depression six or eight miles across each way, in Iowa, west of Nauvoo, Illinois, connected with the first by a westward river channel; apparently worn down from an east and west trough of general curvature, like others southward in the vicinity.

Still beyond the last, to the south-west six or eight miles across broken high lands, is the north end of the second main depression. This has no connection with the first. It is in the State of Iowa, twelve miles north-west of the city of Keokuk. The Desmoines River enters it near this point, from a small channel, partly worn, between the hills from the north-west. The Mississippi River enters it at Keokuk through a channel of evident erosion of a mile wide, from the north-east and north; but without noticeable impression upon it. Its course is south for fifty miles, then turning south-east, with a width of five to eight miles, increasing to from eight to twelve, and a whole length of over a hundred miles.

The Illinois River flows through such depressions; and they exist at various places over the country, but, apparently, are not yet fully traced out by surveys. We find them occupied by creeks and by small rivers, as chance seems to determine.

The eroding power of drainage streams, as indicated by the larger rivers where they have clearly worn out channels, assisted often by rapids, shows no sort of relationship to these spaces where great beds of horizontal solid strata disappear over areas scarcely conformable to the idea of stream erosion. I can point out no proper cause for them, but I must rest in directing attention to the real nature of the facts.*

* I have, to-day, June 21st, 1869, with my friend Dr. Geo. M. Kellogg, examined the old river shingle of angular Keokuk limestone fragments, overlying the Drift formation on the gently sloping river face of the bluff in this city. It is at a height, I suppose, of from sixty to ninety feet above the present river level. And is on the bluff curve toward the second main lake bed above described. This probably shows a former depth of nearly so much to the lake. Dr. Kellogg has heretofore viewed this shingle as indicating an ancient fall at this place. But I see no indications that the fall was not below the lake, a hundred miles away.

TITLES OF COMMUNICATIONS.*

1. On the Genus of extinct Sea-Saurians, *Elasmosaurus*, . . . EDWARD D. COPE.
2. On the Artistic Evidence of the Remote Colonization of the North-Western or American Continent by Maritime People of Distinct Nationalities before the Modern Era, . J. H. GIBBON.
3. Effect of Atmospheric Changes on the Eruptions of the great Geyser of Iceland, ... P. A. CHADBOURNE.
4. Notes on the Defects of Lightning Rods, ... JAMES BUSHEE.
5. On the Boulder Field in Cedar County, Iowa, ... RUSH EMERY.
6. Phases of Glacial Action in Maine at the Close of the Drift Period, . N. T. TRUE.
7. Some Experiments on the Influence of the Physical Condition upon the Personal Equation, in Transit Observations, ... W. A. ROGERS.
8. The Calculation of the Crystalline Form of the Anhydrous Carbonates, Nitrates, Sulphates, Perchlorates, Permanganates, and other Salts of the composition $A B_3 C$, or $A B_4 C$, ... GUSTAVUS HINRICHS.

*The papers under this head were also read; of most no copy has been received for publication; of others it was voted that the title only should be printed. No notice, *even by title*, is taken of articles not approved.

24. Exhibition of the Crania of Boötherium and Castoroides, with Remarks on their Geological Position and their Living Analogues, . .	J. W. Foster.
25. On the Occurrence of Fluor Spar in Southern Illinois, ...	J. W. Foster.
26. On the Refrigeration of Continents, . .	J. W. Foster.
27. On the Occurrence of Tin in Missouri,	J. W. Foster.
28. Migrations of the Indian Family, .	L. H. Morgan.
29. Hough's Barometrograph as applied to the Investigation of the Storm Curve,	J. H. Coffin.
30. Brief Remarks on the Botany, Meteorology, and Geology of Mount Mansfield, Vermont, ...	James Hyatt.
31. Habits and Peculiarities of Plants in South Eastern New York and Vicinity, ...	James Hyatt.
32. Movements in Stratified Rocks, since the Glacial Epoch, ...	James Hyatt.
33. On the Stratigraphical Relations of the Fossil Horse in the United States,	Chas. Whittlesey.
34. On the Formation of Shells and Belemnites, and Phosphates of Iron at Mulica Hill, Gloucester Co., N. J.,	A. B. Engstrom.
35. Notice of some New Vertebrate Remains from the Tertiary of New Jersey, ...	O. C. Marsh.
36. Further Notice of Experiments on Snow and Ice at a Temperature below 32° F.	Edw. Hungerford.
37. Experiment Illustrating the Flow of Glaciers, ...	Edw. Hungerford.
38. The Recent Contributions of Science to the Arts of Dyeing and Printing Woolen Tissues, ...	John L. Hayes.

56. Fuel Resources of Illinois, . . A. H. WORTHEN.

57. On the Physical Geography of the Continent of North America during the different Geological Periods, . . J. S. NEWBERRY.

58. On the Transportation of the Materials forming the Carboniferous Conglomerates, ... J. S. NEWBERRY.

59. On the Surface Geology of the Basin of the Great Lakes and the Upper Mississippi Valley, ... J. S. NEWBERRY.

60. On the Plasticity of Rocks, and Origin of the Structure of the so called Gravestone Slates of California, . W. P. BLAKE.

61. On the Gradual Desiccation of the Western Portions of North America, W. P. BLAKE.

62. Vestiges of Prehistoric places in California, ... W. P. BLAKE.

63. Geological Section of Ohio, . . E. B. ANDREWS.

64. The Hot Term of July, . . . O. N. STODDARD.

65. The Higher Law of Correlation, . O. N. STODDARD

66. On Certain Physical Features of the Mississippi River, ... G. K. WARREN.

67. The Vertebral Type of the Cranium a Quinary one, ... T. C. HILGARD.

68. Extremities of the Skeleton Typically 5, T. C. HILGARD.

69. The Quebec Group in Northern New Hampshire, ... C. H. HITCHCOCK.

70. The Supposed Triassic Foot-marks in Kansas, ... C. H. HITCHCOCK.

71. The Distortions of Pebbles in Conglomerate at Rangely, Maine, . G. L. VOSE.

72. On *Elasmognathus* and its Relations to the Tapiridæ generally, . . . THEODORE GILL.

90. On Two New Fossil Trees, the oldest known, found by Rev. H. HERZER, in the Devonian Rocks of Ohio, . J. S. NEWBERRY.

91. Tides in Lakes, ... H. A. NEWTON.

92. On the Evaporative Power of the Sun near the Base of the Sierra Nevada, in Calaveras County, California, . BENJ. SILLIMAN.

93. On the Tides of Lake Michigan, . W. FERREL.

94. On Redemption Periods of Monetary Values which involve Life Contingencies, ... E. B. ELLIOTT.

95. On the Metrical Unification of International Coinage, ... E. B. ELLIOTT.

96. A Few Facts concerning the Vital Statistics of Amherst College, . . EDW. HITCHCOCK.

97. On the Archæological Value of Certain Ancient Beads, ... L. G. OLMSTEAD.

98. Exhibition of a New Selenograph, . JEROME ALLEN.

99. On a New Method of Measurement Map Drawing for Schools, . . JEROME ALLEN.

100. Glaciers as Extensive and Constant Geologic Agencies, . . . JAMES HYATT.

101. Points in the Geology of Hudson River, JAMES HYATT.

102. On Some of the Causes which Affect the Rapidity of Erosion of Rocks and of River Valleys, . . . WM. P. BLAKE.

103. On the Interpretation of Fossils, . B. W. HAWKINS.

104. Notes on the Origin of Bitumens, together with Experiments upon the Formation of Asphaltum, . . S. F. PECKHAM.

105. Note on Epitrochoidal Teeth, . . WM. WATSON.

106. On the Preservation of Meats, . . JOHN GAMGEE.

EXECUTIVE PROCEEDINGS

OF

THE CHICAGO MEETING, 1868.

HISTORY OF THE MEETING.

THE Seventeenth Meeting of the American Association for the Advancement of Science was held at Chicago, Illinois, commencing on Wednesday, August 5th, and continuing to Wednesday Noon, August 12th.

Two hundred and fifty-nine names are registered in the book by members who attended this meeting. Four hundred and seventy-five new members were chosen, of whom two hundred and sixty-one have signified their acceptance by paying the annual assessment, and, when practicable, signing the Constitution.

One hundred and fifty-one papers were presented, most of which were read, and some of them discussed at great length.

The sessions of the Association were held partly in the Library Hall of the Young Men's Association and other rooms of the same building, and partly in the Lecture Rooms of the First Baptist Church.

At about 10 o'clock A.M. on Wednesday the members were called to order by Ex-President NEWBERRY, who spoke as follows:

Gentlemen of the Association: The time has arrived to which we adjourned at our meeting last year, and it is my duty to call you to order for the organization of the Seventeenth Meeting of the Association. As the last, and perhaps the most agreeeble part of my official duty, I take pleasure in introducing to you the President elect and my successor, Professor B. A. GOULD.

Dr. GOULD then said:

Gentlemen of the Association: It has been for many years the custom of our Association, before entering upon its proceedings, to ask the Divine blessing, and I will call upon the Rev. Dr. R. W. PATTERSON, of this city, to offer prayer.

Prayer was then offered by Dr. PATTERSON.

The Hon. J. Y. SCAMMON then delivered an address of welcome as follows:

MR. PRESIDENT AND GENTLEMEN OF THE AMERICAN ASSOCIATION FOR THE ADVANCEMENT OF SCIENCE:

The pleasant duty of addressing words of welcome to you has been assigned to me. They would come more appropriately from Mr. George C. Walker, the devoted President of the Chicago Academy of Sciences, to whose exertions the city of Chicago is greatly indebted for the present condition of that institution. But as he declined to relieve me of this duty, on his behalf and that of the Chicago Academy of Sciences, the Local Committee and the citizens of the Garden of the Lakes, I bid you welcome; and while we extend to you the hospitalities of the city and its citizens, we thank you for appointing this meeting of the Association in Chicago. We are a republican people, with no boasts of ancestry or hereditary distinction. Our community is composite, representing the nationality of every country in Europe, with a slight trace of Asia, Africa and the Isles of the ocean. In this fact we find occasion for the greatest hopes for the future, for so far as a nation or people represents universal humanity is its own capacity for perfection. The English language is stronger and richer than any other, because it is indebted to many different sources for its form and character, and the power of Great Britain is doubtless greatly owing to the fact that its population is more fully a representative of the various nations of the world than any other country in Europe. If this be so we may hope that the city of Chicago, and the country of which it is the centre, will, in time, present one of the fullest developments of humanity. In this city, the land of which was only surveyed into lots in 1830, and which now has a population of over a quarter of a million, there are more languages spoken, perhaps, than in any other American city, and Northern and Western Europe are most fully represented in its periodical press; and it is fit and becoming that your Association, that has for its objects "the advancement of science by periodical and migratory meetings, to promote intercourse between those who are cultivating science in different parts of North America, to give a stronger and more general impulse, and a more systematic direction to scientific research in our country and to procure for the labors of scientific men increased facilities and a wider usefulness," should hold its meetings in our midst. Our scientific and literary institutions have not much to present beyond the foundation stones, but as *Il n'y a que le premier pas qui coute*, we rejoice that we can show you a beginning not unworthy of a city of thirty years. We make no boast of our position or prowess, though we cannot fail to acknowledge in thankfulness and gratitude the abundance which crowds upon us and the facilities which are at our doors. We are surrounded by the granaries of the world, and if the cattle of the thousand hills are not ours, the herds about us are more than the hills could hold. We look out upon a great interior ocean, upon whose surface float the white sheets of commerce, and we are connected by canals and iron bands in all directions. Our growth has been rapid, yet substantial, and probably no other city ever attained equal importance from voluntary causes in the same space of time. We are an outgrowth and development of the progress and science of the nineteenth century, and we desire, in welcoming you, to recognize the great movement of the last hundred years, which, passing in mighty power from the Old World to the New, penetrates from the coast to the interior, from the surface to the substance, from the apparent to the real, which elevates science from dead matter to the activity of real life, and raises man from a beast of burden to be the director and controller of all things below him, enduring, as it were, physical force with the skill and activity of human hands. Again, Mr. President and gentlemen, I bid you welcome, and assure you that all we can do to make your session among us agreeable and useful shall not be wanting. May your deliberations be harmonious, and the result of far-reaching importance, and may your assembling in this city again not be long delayed, and when we again meet you here, the course of empire, in its rapid march, may have travelled so far that we can no longer call ourselves a Western city. We may still find pleasure in repeating the lines of Bishop Berkley on the prospects of planting arts and learning in America.

> "Westward the course of empire takes its way.
> The four first acts already past.
> A fifth shall close the drama with the day.
> Time's noblest offspring is the last."

This address of Mr. Scammon was acknowledged by President Gould, in behalf of the Members of the Association, as follows:

Mr. Scammon: In behalf of the Association which I have the honor to represent on this occasion, let me express our hearty thanks for the words of welcome we have heard, and for the deeds which, forerunning your words, have given token of their sincerity. It is a source of delight to us that we are assembled in your noble city; and this delight has been enhanced a hundred fold by the cordiality and hospitality with which we have been welcomed. For we all know that this hearty welcome is not due to our possession of those gifts or honors which the world prizes. We are indebted for it to no repute of wealth, or power, or influence. We are hard-working men of science, whose lives, thoughts and energies are given to pursuits which the world deems shadowy and fruitless, and which are certainly unremunerative in the world's goods. We are not aspirants for the honors of the forum, or the tribune, or the market-place. Most of us may not even look to that well-deserved recognition which awaits the successful teacher. But we are fervently and hopefully seeking to extend the bounds of human knowledge, and to contribute, each of us, something at least towards the progress of our race.

We cannot but prize that welcome most, which is offered us because of our aims and struggles in behalf of the cause which we hold dearest. And we find additional gratification because it comes from one whose name is associated with deeds of munificence in this city, and whose liberality has established its observatory, equipped with the largest refracting telescope in use in any public observatory of the world.

Once more, sir, accept our grateful thanks for the many acts of thoughtful kindness shown us by the Local Committee and the citizens of Chicago, and for your cordial words.

Then, addressing the Association, the President said:

Gentlemen of the American Association for the Advancement of Science: The rapidly revolving year has again brought us to the season of our annual convocation; and through your gratifying confidence and kindness it becomes my pleasant office to express, for all and to you all, mutual congratulations on the recurrence of this our time of scientific reinvigoration, and on the happy auspices under which we are gathered together.

In this magnificent city—sprung from the prairie shore like a creation of Aladdin's lamp, and welcoming us with a hospitality and heartiness which could not be surpassed—we may already see omens that the course of science, like that of empire just alluded to, "Westward takes its way." We have convened to-day at a point more westerly than any at which a scientific convention was ever before assembled. Yet here we find an active and earnest scientific spirit, a scientific academy full of vigorous life, doing its part toward the increase of human knowledge and toward disseminating the spirit of scientific inquiry. And in the cordially affectionate welcome given us, both collectively and individually, we rejoice to recognize tokens of regard for those pursuits to which we devote ourselves, and of which we enjoy the privilege of being regarded, for the time, as representatives.

Just twenty years have elapsed since the founders of this Association met together at Philadelphia; a little band, but full of hope and faith. In these two decades the Association has passed through all the stages of feeble infancy, of overweening confidence, of domestic dissension, of malady well-nigh fatal, of distressful bereavement, and of restoration to strength and usefulness. With our present meeting the years of nonage are at an end, and an era of renewed and increased opportunities for good dawns before us.

When, two years ago, we assembled after a period of suspended animation, at the beautiful and thriving city on the shore of Lake Erie, there seemed room for serious doubt whether the calamities which had so seriously tried and imperilled the nation had not destroyed the very life of this Association. Thanks to the large-souled and warm-hearted citizens of Buffalo, our meeting there, although informally called and by no means largely attended, was eminently successful in reviving the old feeling of attachment to the Association; for the recollections of that meeting will always remain associated in our memories with unalloyed pleasure, and it insured for us in the ensuing year a meeting, the success of which has placed the vitality of the Association beyond all peradventure. And the spirit of cordial coöperation, of mutual regard, appreciation and encouragement, of kindly and courteous, yet honest criticism, which formed so marked a characteristic of these two meetings, affords grounds for confident anticipations of future success in that great work for which this organization was established—the Advancement of Science in America.

But, amid all the hopeful auguries and pleasant impressions which surround us here, the dark shadow, rarely absent from human festivals, has not failed to throw its gloom. There are some well remembered and honored countenances, familiar to our eyes a year ago, which we may never more see at our annual gatherings. And we would not fail in some tribute of respect and honor to three earnest workers in science who have, during the past year, been summoned from earth. The venerable botanist of Rochester, who for so long a period was never absent from our meetings,—the laborious and faithful astronomer of the Washington Observatory, who added to the lyre of heaven so many strings,—the accomplished engineer of Schenectady, once the General Secretary of the Association,—will long retain a place in your memories, and in the annals of the sciences to which they faithfully devoted themselves. And it would ill beseem us did we fail to recall with reverence the names of Chester Dewey, Ferguson, and William Mitchell Gillespie.

We now commence the Seventeenth Meeting of the American Association for the Advancement of Science. Let us not fail, through all its stages, to remember that its objects are best promoted by the union of kindly sympathy with ingenuous criticism. We seek the sharp encounter of mind with mind, yet not that any energy or power may be curtailed or rendered less effective, but rather that, as in the encounter of flint with steel, light may be evoked, where obscurity prevailed before.

Through the personal intercourse which these meetings promote between students in kindred departments of science, our meetings are undoubtedly even more serviceable to the great end in view than through the information contained in the formal communications. The experience of many years in our own land, as well as in Germany, England and France, shows that by far the greatest good is done outside the walls of the section-rooms. Ideas exchanged, suggestions offered, asperities softened, coöperations established, are the legitimate and constant fruits of these meetings.

Yet it is not only from the reciprocal exchange of ideas between individuals, and the public presentation of papers to the meetings, that the benefits of the Association flow. One of its chief functions is, by the migratory character of its meetings, to foster and diffuse a love of science and a taste for the investigation of nature. It is not among our functions to disseminate knowledge, but rather to attain it and to procure its attainment, and to conquer new truths from the domain of the unknown.

May I be pardoned if I presume to suggest yet another method by which the objects of the Association may be promoted, although it is one which for many reasons we have never yet essayed, and which until a very recent date it would have been both undesirable and futile to attempt. I refer to the institution of special researches in science by means of the funds of the Association, when these, by careful nursing and judicious management, shall permit of such an application.

Fifteen years ago, at the Cleveland meeting, this subject was discussed, and the opinion was found to be unanimous, that any attempt of the kind would then be injudicious. The progress of years has now changed the whole aspect and bearings of the case, and if a few hundred dollars should now be annually set aside as a fund to be placed in the hands of trustees, and the income of this ultimately be devoted to the prosecution of researches which would probably not otherwise be undertaken, a good work might be accomplished for the Advancement of Science. The great debt which many branches of science owe to the British Association for work of various kinds thus performed I need not recall to your minds. What might we not thus accomplish for the new science of meteoric astronomy, in the creation of which we may claim so large a share for our own countryman, Professor Newton? What might we not thus do for terrestrial magnetism? The expense of equipping and maintaining for five years a merchant vessel, provided with a few competent observers and instruments, would determine the magnetic constants for every point of the whole earth, and probably result in the development of facts and laws by which all the physics of the globe, and all geology, would be essentially advanced, — manifold more indeed than by the continuance in activity of every magnetic observatory now existing. Ancient astronomical observations might be reduced, and a contribution to stellar astronomy thus obtained, of incomparably greater value than would be afforded by observations made to-day. Tables of physical constants might be prepared, expeditions of exploration might be equipped, or costly yet important experiments carried out. That our Association will ultimately accomplish such work as this is among my most earnest convictions, as it is among my fondest hopes.

But I will not detain you longer. Let us pass to our work, bringing each of us what little we have been able to attain for the increase of human knowledge, in the hope that our several contributions may, when added to those of the past and future, take shape and form, becoming centres of crystallization and growth for truths yet unattained. Let us strive to promote our own progress and that of others, in the great work to which we have dedicated our lives and such fortunes as we may possess, by mutually inciting each other to new effort, and by invoking that mental condition which science ought to foster,—humility before the vast unknown, mingled with that confidence which should animate the explorer.

Gentlemen, let me offer my sincere thanks for the great honor which your partiality has conferred upon me, and of which it shall be my aim to render myself more worthy hereafter. In the fulfilment of the duties of my office I shall look to you for aid and counsel, and shall rely upon your kindness to palliate my errors, and to attribute them, as you certainly may do, to no motives inconsistent with an earnest desire for impartiality, and for the best welfare of the Association.

The Association then proceeded to the election, by ballot, of six additional members of the Standing Committee, according to the requirement of Rule 4 of the Constitution. The names of those chosen are printed elsewhere with the names of the other members of that Committee.

The Association voted to hold its next meeting at Salem, Massachusetts, beginning on Wednesday, August 18, 1869. The officers elected for the next meeting are:

Colonel J. W. Foster, of Chicago, *President;* Professor O. N. Rood, of New York, *Vice-President;* Professor O. C. Marsh, of New Haven, *General Secretary;* Dr. A. L. Elwyn, of Philadelphia, *Treasurer.* Professor Joseph Lovering was elected, at Burlington, *Permanent Secretary* for another term of two years, commencing with the Chicago Meeting.

On Wednesday evening a general meeting was held in Library Hall, when President F. A. P. Barnard delivered his address as retiring President, which had been postponed from the last year; and, on Thursday evening, a general meeting was held in the same place, when the President, B. A. Gould, delivered an eulogy upon the late A. D. Bache.

A communication was received from Professor Benjamin Peirce, Superintendent of the United States Coast Survey, in relation to the changes now occurring in the star *Eta Argus* and the surrounding nebula, and recommending to the Association to express its opinion on the extreme importance of observations upon this nebula to be made in South America. The subject was referred to a committee, which will be found in another part of this volume.

Professor F. A. P. Barnard proposed the appointment of a committee to express the congratulations of this Association to Dr. Ehrenberg, of Berlin, on his approaching Jubilæum, and enforced his recommendation with the following remarks: "It has long been the custom in Europe, whenever any eminent contributor to science approaches the half century of his doctorate, to send to him addresses and testimonials, expressive of the respect and honor in which he is held. This is often done by individuals, but all organized associations are accustomed to send their resolutions. The American Academy of Arts and Sciences, in Boston, has already designated two of its members, Dr. Asa Gray and Professor Joseph Lovering, to present *in person* the congratulations of that Academy. In this case the tribute is peculiarly merited. Few men have done so much for the department of science in which the proposed recipient of honor has been engaged. He found the use of the microscope, in the study of natural history, almost barren. He stimulated it to its present great and high utility, opening such a world of investigation that even the instrument itself was perfected in the contemplation of it. The result has been the disclosure of a universe to be studied as multiform as that which the telescope reveals. No man ever presented to the world so much new matter as Ehrenberg in his book on *Infusoria*. He is now seventy-four years of age, and to his great labors he has sacrificed his eyes, and, by a recent accident, he has lost the power of locomotion. It appears, in view of these facts that America should join hands with Europe in paying a tribute of respect and gratitude to the man who by his great labor has procured such great results."

Many of the members in attendance accepted the private hospitality generously offered by families in the city of Chicago.

On Saturday, August 8, after the session of the morning, which was continued in the evening, the members of the Association and their ladies, accompanied by many ladies and gentlemen of Chicago, by invitation of the Local Committee, embarked on board the Steamer *Orion* and enjoyed an excursion upon Lake Michigan, visiting the Crib, and partaking of a bountiful lunch on board the Steamer.

During the meeting of the Association in Chicago, the members and their friends were elegantly entertained, on successive evenings, by Messrs. John B. Drake of the Tremont House, Wm. E. Doggett, N. S. Davis, and J. Y. Scammon, at their private residences, and Gage Brothers and Walters of the Sherman House.

Through the extraordinary liberality of the Directors of many of the railroads which centre in Chicago, the members of the Association had an opportunity after the final adjournment of the meeting, to make excursions to points of scientific interest, and not a few availed themselves of the privilege to visit the Coal Valley near Rock Island, the Mines of Lake Superior, or the newly planted germs of a city on the Missouri River, OMAHA. A small party proceeded from Omaha, by the invitation of Hon. W. B. Ogden, and Capt. J. B. Turner, over the whole length of the Union Pacific Railroad to a point more than sixty miles beyond Benton, and, on the afternoon of August 17, after dining in the construction-train of General Casement, rode over a mile of road which had been finished with the rails which their own train had brought forward two hours before.

RESOLUTIONS ADOPTED.

ON THE CINCINNATI OBSERVATORY.

Resolved, That this Association cordially sympathizes with the effort now making by the Astronomical Society and the citizens of Cincinnati, to resuscitate the Cincinnati Observatory, and earnestly desires its success.

ON THE JUBILÆUM OF DR. EHRENBERG.

Whereas this Association is apprized that the veteran naturalist, CHRISTIAN GOTTFRIED EHRENBERG, will complete during the present year the fiftieth year of his Doctorate in Medicine, and whereas the Association sincerely desires to express their high appreciation of the eminent services rendered to Natural History, and especially to the knowledge of microscopic organisms, by this distinguished and laborious investigator and observer; therefore

Resolved, That a committee of three* be appointed to prepare an address to be presented to Dr. Ehrenberg, on the occasion of his approaching Jubilæum, congratulating him on the long and honorable career which it has been permitted him to enjoy, and assuring him of the deep veneration and respect in which his name is held in America.

Resolved, That the address, when prepared, be forwarded to the Hon. George Bancroft, Envoy Extraordinary and Minister Plenipotentiary of the United States of America to the Confederation of Northern Germany, with the request that he present the same to Dr. Ehrenberg, in the name of this Association, on the occasion of his Jubilæum, on the 5th of November next.

ON AN INTERNATIONAL COPYRIGHT.

Resolved, That it is the conviction of this Association that no product of human industry is, in a stricter sense, the rightful property of the producer than that which embodies in written form the results of intellectual labor.

Resolved, That literary property, in whatever country produced, ought, equally with every other description of property, to be under the protection of law, and to enjoy such protection equally in all countries.

Resolved, That this Association cordially approves and endorses the efforts which are making by the *Copyright Association*, recently organized for the United States of America, to secure the recognition by all governments of the rights of authors to their literary productions, and sincerely desires that these efforts may meet with early and complete success.

ON THE ESTABLISHMENT OF AN OBSERVATORY ON THE LINE OF THE UNION PACIFIC RAILROAD.

Resolved, That this Association recommends to the attention of those who would make intelligent and munificent endowments of scientific institutions, the importance of an Astronomical Observatory at some point on the Pacific Railroad between Nebraska and the Pacific Coast, and at as high an altitude as possible, where the clearness of the atmosphere, and the great number of cloudless days, would ensure remarkable and unsurpassed opportunities for astronomical observations.

* Afterwards increased to seven.

VOTES OF THANKS.

Resolved, That we owe to the accomplished Chairman of the Local Committee and to its indefatigable Secretary, and to the assiduities of the various subcommittees, a very large share of the order, success, and the unusual personal enjoyments which have characterized this meeting.

Resolved, That the thanks of this Association be given to the Young Men's Library Association, to the Caledonian Club, and to the Trustees and Officers of the First Baptist Church in Chicago, for the ample accommodations which they have freely placed at our disposal.

Resolved, That the thanks of this Association be presented to the editors of this city for the full reports of its proceedings published in their columns.

Resolved, That the thanks of the Association be presented to the Academy of Sciences, the Historical Society, the Young Men's Library Association, the Board of Trade, and to all other bodies, scientific, literary or commercial, who have freely opened to our use their rooms and collections, for the many courtesies which have contributed so much to the success of the meeting and the enjoyment of the members.

Resolved, That we heartily appreciate the hospitality which has been generously extended to the members of the Association, individually and collectively, by the citizens of Chicago, and the opportunity enjoyed by us of social intercourse with the ladies and gentlemen of the place at the receptions, in hotels and private houses, which welcomed our arrival here, which have cheered our leisure moments from day to day, and which have continued even to the hour of our last farewell.

Resolved, That the members of this Association acknowledge their great indebtedness to the Directors of many of the Railroads, over which they have passed in coming from their homes to this distant place of meeting, for the liberal terms upon which they have been carried; and that they are placed under peculiar obligations to the Directors of the Railroads which diverge from Chicago, and especially of the Chicago and Northwestern Railroad, for their extraordinary liberality in furnishing free passes to Rock Island, Omaha, Lake Superior, and other points of great scientific interest.

Resolved, That the thanks of the Association be presented to the retiring President, for the dignity, ability and impartiality with which he has filled the chair during the present meeting.

Railroad and Steamboat Corporations which have charged only HALF FARE *to members of the Association in attendance on the Chicago meeting.*

Chicago and Northwestern,
Chicago and Rock Island,
Chicago, Burlington and Quincy,
Chicago and Alton,
Illinois Central,
Michigan Central,
Michigan Southern,
Chicago, Columbus, and Indiana Central,
Grand Trunk,
Great Western,
Louisville and Nashville,
Mobile and Ohio,
Mississippi Central,
New Orleans and Jackson,
South Carolina,
Orange and Alexandria,
Wilmington and Weldon,
New Jersey Central,
New Jersey,
Philadelphia and Erie,
Pennsylvania,
New York and Erie,
Connecticut River,
Eastern of Massachusetts,
Boston and Montreal,
Boston and Ogdensburg,
Boston and Maine.

REPORT OF THE PERMANENT SECRETARY.

This report includes the executive business which belongs to the interval between the commencement of the Burlington meeting (August 21, 1867) and that of the Chicago meeting (August 5, 1868).

The Association now numbers four hundred and fifteen members. Only fourteen individuals joined the Association at Burlington. One hundred and thirty-two old members were struck from the list, on account of three years' delinquencies in the payment of assessments, their aggregate indebtedness amounting to eleven hundred and eighty-eight dollars ($1188).

The financial condition of the Association is as follows:

Between August 21, 1867 (the first day of the Burlington meeting) and August 5, 1868 (the first day of the Chicago meeting), the income of the Association, *including the balance from last year's account*, was fourteen hundred and twenty-two dollars and seventeen cents ($1422.17), of which fifty-two dollars and fifty cents accrued from the sale of the printed Proceedings, and the remainder from the annual assessments.

The expenses of the Association during the same period were twelve hundred and sixty-two dollars and sixty-one cents, which may be apportioned thus:

Cost of paper, printing and binding the Burlington volume of Proceedings, five hundred and seven dollars and forty-seven cents, ...	$507.47
Charges connected with the Buffalo and Burlington meetings two hundred and nine dollars and ninety-two cents, . . .	209.92
Salary of the Permanent Secretary, five hundred dollars, . .	500.00
Postage, stationery, express, etc., ...	45.22
	$1262.61

The details may be found in the cash account of the Secretary, which is herewith submitted as a part of his report. The balance in the hands of the Permanent Secretary, August 5, 1868, is one hundred and fifty-nine dollars and fifty-six cents ($159.56). There is no balance in the hands of the Treasurer.

JOSEPH LOVERING,
Permanent Secretary.

Chicago, August 5, 1868.

CASH ACCOUNT OF THE

Dr. AMERICAN ASSOCIATION *in*

Bigelow's bill for programmes,	$3.00
Harris's bill as clerk,	16.37
Postage,	27.00
Ripley's bill for circulars,	12.00
Carter's bill for envelopes,	1.25
Smithsonian Institution for express,	3.40
J. H. Abbott, for binding,	2.00
Harris's bill for copying records,	13.25
Clerk for preparing circulars,	5.00
Clarke, for twenty-one reams of paper,	115.50
Kilburn, for wood-cuts,	6.00
Paper for cover,	6.48
Paper and envelopes,	3.75
Ripley, for printing,	8.50
Paper for copying records,	1.80
Abbott, for binding Burlington Proceedings,	29.99
Express charges,	9.82
Expenses of Newport and Buffalo Meetings,	150.00
Salary of Permanent Secretary,	500.00
Dakin's bill for printing Proceedings,	347.50
	$1262.61
Balance to next account,	159.56
	$1422.17

PERMANENT SECRETARY.

Account with Joseph Lovering.	Cr.
Balance from last account, ...	$422.90
Assessments (from No. 421 to No. 651 of Cash Book) including the sale of Proceedings, ...	999.27
	$1422.17

List of European Institutions to which Copies of Volume XVI of the Proceedings of the American Association were distributed by the Permanent Secretary in 1868.

Stockholm,—Kongliga Svenska Vetenskaps Akademien.
Copenhagen,—Kongel. danske Vidensk. Selskab.
Moscow,—Société Impériale des Naturalistes.
St. Petersburg,—Académie Impériale des Sciences.
" " Kais. Russ. Mineralogische Gesellschaft.
" " Observatoire Physique Centrale de Russie.
Pulkowa,—Observatoire Imperiale.
Amsterdam,—Académie Royale des Sciences.
" Genootschap Natura Artis Magistra.
" Zoölogical Garden.
Haarlem,—Hollandsche Maatschappij der Wettenschappen.
Leyden,—Musée d'Histoire Naturelle.
Altenburg,—Naturforschende Gesellschaft.
Berlin,—K. P. Akademie der Wissenschaften.
" Gesellschaft für Erdkunde.
Bonn,—Naturhist. Verein der Preussisch. Rheinlandes, &c.
Breslau,—K. L. C. Akademie der Naturforscher.
Dresden,—K. L. C. Deutsche Akademie der Naturforscher.
Franckfurt,—Senckenbergische Naturforschende Gesellschaft.
Freiburg,—Königlich-Sächsische Bergakademie.
Göttingen,—Königl. Gesellschaft der Wissenchaften.
Hamburg,—Naturwissenschaftlicher Verein.
Hannover,—Die Naturhistorische Gesellschaft.
Leipsic,—Königlich Sächsische Gesellschaft der Wissenschaften.
Munich,—K. B. Akademie der Wissenschaften.
Prag,—K. Böhm. Gesellschaft der Wissenschaften.
Stuttgart,—Verein für Vaterländische Naturkunde.
Vienna,—K. Akademie der Wissenschaften.
" K. K. Geographischen Gesellschaft.
" Geologischen Reichsanstalt.
Württemburg,—Der Verein für Vaterländische Naturkunde.
Basel,—Naturforschende Gesellschaft.
Bern,—Allgemeine Schweizerische Gesellschaft.
" Naturforschende Gesellschaft.
Geneve,—Société de Physique et d'Histoire Naturelle.
Neuchatel,—Société des Sciences Naturelles.
Bruxelles,—Académie Royale des Sciences, &c.
Cherbourg,—Société Académique.
Dijon,—Académie des Sciences, &c.
Liège,—Société Royale des Sciences.

Lille, — Société Nationale des Sciences, de l'Agriculture et des Arts.
Paris, — Institut de France.
" Société Philomatique.
" Société Météorologique de France.
Turin, — Accademia Reale delle Scienzie.
Madrid, — Real Academia de Ciencias.
Cambridge, — Cambridge Philosophical Society.
Dublin, — Royal Irish Academy.
Edinburgh, — Royal Society.
London, — Board of Admiralty.
" East India Company.
" Museum of Practical Geology.
" Royal Society.
" Royal Astronomical Society.
" Royal Geographical Society.
Manchester, — Literary and Philosophical Society.
Batavia, — Société des Arts et des Sciences.

INDEX.

ERRATA.

Owing to the non-receipt of proofs of communications 6 and 7 of Sect. A by the authors until long after they were printed, the following errors were not corrected:

Page 124, line 6, for "$a \sin n.\ b \cos n.$" read $a \sin z.\ b \cos z$; line 8, for "given threads," *read* prime vertical; line 18, for "Bruinoso," *read* Brünow; line 28, for

$$\text{“}I = \frac{i}{\sin\phi \cos\delta \sin(\theta' - T)}\text{”} \quad \textit{read} \quad I = \frac{i}{\sin\phi \cos\delta \sin(\theta' - \tfrac{1}{2}I)}.$$

Page 125, line 2, to first line of formula *add* $\cot\theta'$; line 3, dele the "θ" before $+\frac{a}{\sin\phi}$; line 9, for "969,961" *read* 9.69761; head line of table over the last three columns of figures, for ° ' " *read* h. m. s.; T' I ½ I θ — ½ I ⌣ c should be the headings of the columns in the example.

Page 126, in table, after "copper" for "Paramagnetic" etc., *read* Diamagnetic.

" 127, first word in 10th ¶, for "Heating" *read* Heat in; In same ¶ last line should read diamagnetic like bismuth.

Page 128, line 15, for "wherein," *read* cohesion.

The author of communication 25 of Sect. A wishes the following corrections made:

Page 248, line 9, for "four hundred degrees" *read* five hundred and five; line 10, for "$\sqrt{400 \times 224}$" *read* $\sqrt{505 \times 224}$; and in the same line for "four hundred and eighty feet" *read* nine hundred and thirty-three feet; line 11, after the word "second" insert the following as a foot note:

The actual velocity of sound in water at the temperature of 46°.6 F., = about 505° above absolute zero, is 4 708 feet per second, as determined, in 1827, from experiments in the Lake of Geneva, by Colladon and Sturm. The excess of the velocity of aqueous atoms, as calculated from the mechanical equivalent of heat, may be a consequence of the ellipticity of the atomic orbits, the lesser being the mean velocity when referred to a continuous direction.

Page 251, line 6, for "four hundred degrees" *read* five hundred and five degrees; line 11, for "$\sqrt{400}$" *read* $\sqrt{505°}$; and for "270 000" *read* 300 000; line 13, for "$\frac{3}{270000}$" *read* $\frac{3}{300000}$.

NOTE.—It is proper to state here that communication 8 of Sect. A, and 2 and 3 of Sect. B, were printed without a revise having been received from the authors. It is through no fault of the Editor that the authors did not receive their proofs in time, as proofs and letters were sent in each case, and the type was kept standing until it became necessary to send the matter to press.

The Editor would take this opportunity to impress every member of the association with the necessity of writing their full post office address on their manuscripts.

PROCEEDINGS

OF

THE AMERICAN ASSOCIATION

FOR THE

ADVANCEMENT OF SCIENCE.

EIGHTEENTH MEETING,

HELD AT

SALEM, MASSACHUSETTS,

AUGUST, 1869.

CAMBRIDGE:
PUBLISHED BY JOSEPH LOVERING.
1870.

EDITED BY

JOSEPH LOVERING,

Permanent Secretary.

SALEM:

Essex Institute Press.

TABLE OF CONTENTS.

COMMUNICATIONS.

A. MATHEMATICS, PHYSICS, AND CHEMISTRY.

I. MATHEMATICS AND ASTRONOMY.

II. MECHANICS.

III. ASTRONOMY.

IV. ELECTRICITY.

V. CHEMISTRY.

B. NATURAL HISTORY.

I. GEOLOGY AND PALÆONTOLOGY.

II. ZOOLOGY.

III. BOTANY.

IV. ETHNOLOGY.

EXECUTIVE PROCEEDINGS.

OFFICERS OF THE ASSOCIATION

AT

THE SALEM MEETING.

J. W. FOSTER, *President.*

O. N. ROOD, *Vice-President.*

JOSEPH LOVERING, *Permanent Secretary.*

O. C. MARSH, *General Secretary.*

DR. A. L. ELWYN, *Treasurer.*

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O. N. ROOD,
JOSEPH LOVERING,
O. C. MARSH,
B. A. GOULD,
CHARLES WHITTLESEY,
A. P. ROCKWELL,
A. L. ELWYN.

AS CHAIRMEN OF THE SECTIONAL COMMITTEES.

JOSEPH HENRY,
LOUIS AGASSIZ.

FROM THE ASSOCIATION AT LARGE.

ALEXIS CASWELL,
T. S. HUNT,
W. C. KERR,
J. S. NEWBERRY,
BENJAMIN PEIRCE,
JOHN TORREY.

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WILLIAM SUTTON, *Treasurer.*

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A HUNTINGTON,
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J. C. LEE,
G. B. LORING,
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J. C. OSGOOD,
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S. E. PEABODY,
G. PERKINS,
G. A. PERKINS,
W. P. PHILLIPS,
G. D. PHIPPEN,
C. H. PRICE,
R. S. RANTOUL,
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C. A. ROPES,
G. P. RUSSELL,
B. H. SILSBEE,
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R. P. WATERS,
B. WEBB, Jr.,
B. A. WEST,
S. G. WHEATLAND,
H. L. WILLIAMS,
E. B. WILLSON.

OFFICERS OF THE SECTIONS.

SECTION A.

JOSEPH HENRY, *Chairman.*
HENRY WURTZ, *Secretary* up to the 4th day.
DAVID MURRAY, *Secretary* for the rest of the session.

Sectional Committee.

G. F. BARKER,
WILLIAM A. ROGERS,
DAVID MURRAY, up to the 4th day.
JOHN FOSTER, for the rest of the session.

SECTION B.

LOUIS AGASSIZ, *Chairman.*
T. STERRY HUNT, *Secretary* up to the 3d day.
G. A. LEAKIN, *Secretary* for the rest of the session.

Sectional Committee.

O. C. MARSH, E. D. COPE, A. C. HAMLIN.

SUB-SECTION C OF SECTION B.

E. G. SQUIER, *Chairman* up to the 3d day.
ARNOLD GUYOT, *Chairman* for rest of the session.
WILLIAM H. DALL, *Secretary.*

SUB-SECTION D OF SECTION B.

J. E. GAVIT, *Chairman.*
EDWIN BICKNELL, *Secretary.*

SPECIAL COMMITTEES.

A. Committees continued from former Meetings.

1. *Committee to Report in Relation to Uniform Standards in Weights, Measures, and Coinage.*

F. A. P. Barnard,
John F. Frazer,
J. H. Gibbon,
Wolcott Gibbs,
B. A. Gould,
Joseph Henry,
J. E. Hilgard,
John LeConte,
H. A. Newton,
Benjamin Peirce,
W. B. Rogers,
J. L. Smith,
John Torrey.

B. New Committees.

1. *Committee to Audit the Accounts of the Permanent Secretary and the Treasurer.*

B. A. Gould,
C. S. Lyman.

2. *Committee with whom the Permanent Secretary may advise in regard to the publication of the Salem Proceedings.*

Benjamin Peirce.
Louis Agassiz.

3. *Committee to act with the Standing Committee in nomination of officers for the Troy Meeting.*

Section A.*	Section B.
E. W. Blake,	W. P. Blake,
E. B. Elliott,	C. F. Hartt,
W. A. Norton,	W. C. Kerr,
C. A. Young,	R. P. Stevens.

4. *Committee to select the time and place of the next meeting.*

J. S. Newberry,
T. S. Hunt,
Joseph Henry,
L. H. Morgan,
John Torrey.

5. *Committee for assisting the Local Committee in making arrangements with the Railroads for transporting the members to and from the next meeting.*

J. H. Devereux,
Samuel M. Felton,
J. W. Foster,
M. D. Hanover,
G. B. Loring,
L. H. Morgan,
William B. Ogden,
James D. Warner.

*E. B. Elliott and C. A. Young were chosen to fill the vacancies caused by the absence of G. W. Hough and E. N. Horsford, who were originally chosen.

OFFICERS OF THE TROY MEETING.

WILLIAM CHAUVENET, *President.*
T. S. HUNT, *Vice-President.*
JOSEPH LOVERING, *Permanent Secretary.*
C. F. HARTT, *General Secretary.*
A. L. ELWYN, *Treasurer.*

Standing Committee.

WILLIAM CHAUVENET,
T. S. HUNT,
JOSEPH LOVERING,
C. F. HARTT,
J. W. FOSTER,
O. N. ROOD,
O. C. MARSH,
A. L. ELWYN.

Local Committee.

JOHN A. GRISWOLD, *Chairman.*
GEORGE C. BURDETT, *First Vice-Chairman.*
P. V. HAGNER, *Second Vice-Chairman.*
BENJ. H. HALL, *General Secretary.*
H. B. NASON, *Corresponding Secretary.*
ADAM R. SMITH, *Treasurer.*

E. W. ARMS,
MILES BEACH,
E. W. BOUGHTON,
IRVING BROWNE,
H. BURDEN,
J. A. BURDEN,
E. CORNING, JR.,
D. COWEE,
G. H. CRAMER,
C. DROWNE,
C. E. DUTTON,
WILLIAM FENTON,
J. L. FLAGG,
JAMES FORSYTH,
JAMES HALL,
W. H. HART,
J. C. HEARTT,
J. S. HEARTT,
A. L. HOLLEY,
C. R. INGALLS,
A. G. JOHNSON,
G. B. KELLOGG,
JUSTIN KELLOGG,
WILLIAM KEMP,
J. S. KNOWLSON,
J. H. C. LAJOIE,
F. B. LEONARD,
H. C. LOCKWOOD,
J. ROMEYN,
H. ROUSSEAU,
W. P. SEYMOUR,
W. A. SHEPARD,
N. B. SQUIRES,
G. H. STARBUCK,
F. S. THAYER,
W. A. THOMPSON,
JAMES THORN,
DUDLEY TIBBITS,
C. W. TILLINGHAST,
M. I. TOWNSEND.
J. I. TUCKER,
D. T. VAIL,

Local Committee (Continued).

J. M. Francis,	C. McMillan,	S. M. Vail,
J. W. Fuller,	M. L. Marks,	M. R. Vincent,
E. T. Gale,	G. W. Maynard,	R. H. Ward,
Uri Gilbert,	G. G. Moore,	G. B. Warren,
H. Gnadendorff,	A. B. Morgan,	J. M. Warren,
Hannibal Green,	G. P. Ogden,	S. E. Warren,
Robert Green,	J. B. Parmenter,	W. P. Warren,
C. O. Greene,	C. E. Patterson,	D. A. Wells,
Dascom Greene,	J. B. Pierson,	H. B. Whiton,
C. Griswold,	A. E. Powers,	L. Wilder,
W. Gurley,	J. R. Prentice,	J. H. Willard,
	D. Robinson,	W. H. Young.

Local Sub-Committees.

On Receptions.

Martin I. Townsend, James Forsyth, H. B. Nason, C. W. Tillinghast, Chester Griswold.

On Finance.

Jos. W. Fuller, James A. Burden, C. O. Greene, William Gurley, Uri Gilbert, David Cowee.

On Lodging and Entertainment.

William Kemp, Wm. A. Thompson, Wm. H. Young, Ezra W. Boughton, James R. Prentice.

On Excursions.

John A. Griswold, Wm. A. Shepard, F. S. Thayer, J. L. Flagg, Walter P. Warren.

On Rooms for Meetings.

John H. Willard, Miles Beach, Giles B. Kellogg, J. Romeyn, Jonas S. Heartt.

On Invitations, Correspondence, and Printing.

B. H. Hall, Irving Browne, C. E. Dutton, G. W. Maynard, A. E. Powers.

On Railroads.

E. Thompson Gale, Geo. C. Burdett, Daniel Robinson, Geo. H. Cramer, A. L. Holley.

MEETINGS OF THE AMERICAN ASSOCIATION FOR THE ADVANCEMENT OF SCIENCE.

Meeting.	Date.	Place.	President.	Vice-President.	General Secretary.	Permanent Sec'y.	Treasurer.
1st,	Sept. 20, 1848,	Philadelphia, Pa.,	W. C. Redfield,		Walter R. Johnson,		Jeffries Wyman.
2d,	Aug. 14, 1849,	Cambridge, Mass.,	Joseph Henry,		E. N. Horsford,		A. L. Elwyn.
3d,	March 12, 1850,	Charleston, S. C.,	A. D. Bache,*		L. R. Gibbes,*		St. J. Ravenel.*
4th,	Aug. 19, 1850,	New Haven, Conn.,	A. D. Bache,		E. C. Herrick,		A. L. Elwyn.
5th,	May 5, 1851,	Cincinnati, Ohio,	A. D. Bache,		W. B. Rogers,	S. F. Baird,	A. L. Elwyn.
6th,	Aug. 19, 1851,	Albany, N. Y.,	Louis Agassiz,		W. B. Rogers,	S. F. Baird,	A. L. Elwyn.
7th,	July 28, 1853,	Cleveland, Ohio,	Benj. Peirce,		J. D. Dana,	S. F. Baird,	A. L. Elwyn.
8th,	April 26, 1854,	Washington, D. C.,	J. D. Dana,		J. Lawrence Smith,	Joseph Lovering,	J. L. LeConte.*
9th,	Aug. 15, 1855,	Providence, R. I.,	John Torrey,		Wolcott Gibbs,	Joseph Lovering,	A. L. Elwyn.

*In the absence of the regular officer.

MEETINGS OF THE AMERICAN ASSOCIATION FOR THE ADVANCEMENT OF SCIENCE.

Meeting.	Date.	Place.	President.	Vice-Presidents.	General Secretary.	Permanent Sec'y.	Treasurer.
10th,	Aug. 20, 1856,	Albany, N. Y.,	James Hall,		B. A. Gould,	Joseph Lovering,	A. L. Elwyn.
11th,	Aug. 12, 1857,	Montreal, C. E.,	J. W. Bailey,	Alexis Caswell,	John LeConte,	Joseph Lovering,	A. L. Elwyn.
12th,	April 28, 1858,	Baltimore, Md.,	Alexis Caswell,*	John E. Holbrook,	W. M. Gillespie,	Joseph Lovering,	A. L. Elwyn.
13th,	Aug. 3, 1859,	Springfield, Mass.,	S. Alexander,	Edward Hitchcock,	William Chauvenet,	Joseph Lovering,	A. L. Elwyn.
14th,	Aug. 1, 1860,	Newport, R. I.,	Isaac Lea,	B. A. Gould,	Joseph LeConte,	Joseph Lovering,	A. L. Elwyn.
15th,	Aug. 15, 1866,	Buffalo, N. Y.,	F. A. P. Barnard,	B. A. Gould,	Elias Loomis,	Joseph Lovering,	A. L. Elwyn.
16th,	Aug. 21, 1867,	Burlington, Vt.,	J. S. Newberry,	Wolcott Gibbs,	C. S. Lyman,	Joseph Lovering,	A. L. Elwyn.
17th,	Aug. 5, 1868,	Chicago, Ill.,	B. A. Gould,	Charles Whittlesey,	Simon Newcomb,*	Joseph Lovering,	A. L. Elwyn.
18th,	Aug. 18, 1869,	Salem, Mass.,	J. W. Foster,	O. N. Rood,	O. C. Marsh,	F. W. Putnam,*	A. L. Elwyn.

*In the absence of the regular officer.

CONSTITUTION OF THE ASSOCIATION.*

OBJECTS.

THE Association shall be called THE AMERICAN ASSOCIATION FOR THE ADVANCEMENT OF SCIENCE.

The objects of the Association are, by periodical and migratory meetings, to promote intercourse between those who are cultivating science in different parts of the United States, to give a stronger and more general impulse and a more systematic direction to scientific research in our country, and to procure for the labors of scientific men increased facilities and a wider usefulness.

MEMBERS.

RULE 1. Any person may become a member of the Association upon recommendation in writing by two members, nomination by the Standing Committee, and election by a majority of the members present.

OFFICERS.

RULE 2. The officers of the Association shall be a President, Vice-President, General Secretary, Permanent Secretary, and Treasurer. The President, Vice-President, General Secretary, and Treasurer shall be elected at each meeting for the following one; — the three first named officers not to be reeligible for the next two meetings, and the Treasurer to be reëligible as long as the Association may desire. The Permanent Secretary shall be elected at each second meeting, and also be reëligible as long as the Association may desire.

* Adopted August 25, 1856, and ordered to go into effect at the opening of the Montreal Meeting. Amended at Burlington, August, 1867, and at Chicago, August, 1868.

MEETINGS.

RULE 3. The Association shall meet, at such intervals as it may determine, for one week, or longer,—the time and place of each meeting being determined by a vote of the Association at the previous meeting; and the arrangements for it shall be intrusted to the officers and the Local Committee.

STANDING COMMITTEE.

RULE 4. There shall be a Standing Committee, to consist of the President, Vice-President, Secretaries, and Treasurer of the Association, the officers of the preceding year, the permanent Chairman of the Sectional Committees, after these shall have been organized, and six members present from the Association at large, who shall have attended any of the previous meetings, to be elected upon open nomination by ballot on the first assembling of the Association. A majority of the whole number of votes cast, to elect. The General Secretary shall be Secretary of the Standing Committee.

The duties of the Standing Committee shall be,—

1. To assign papers to the respective sections.

2. To arrange the scientific business of the general meetings, to suggest topics, and arrange the programmes for the evening meetings.

3. To suggest to the Association the place and time of the next meeting.

4. To examine, and, if necessary, to exclude papers.

5. To suggest to the Association subjects for scientific reports and researches.

6. To appoint the Local Committee.

7. To have the general direction of publications.

8. To manage any other general business of the Association during the session, and during the interval between it and the next meeting.

9. In conjunction with four from each Section, to be elected by the Sections for the purpose, to make nominations of officers of the Association for the following meeting.

10. To nominate persons for admission to membership.

11. Before adjourning, to decide which papers, discussions, or other proceedings shall be published.

SECTIONS.

Rule 5. The Association shall be divided into two Sections, and as many sub-sections as may be necessary for the scientific business, the manner of division to be determined by the Standing Committee of the Association. The two Sections may meet as one.

SECTIONAL OFFICERS AND COMMITTEES.

Rule 6. On the first assembling of the Section, the members shall elect upon open nomination a permanent Chairman and Secretary, also three other members, to constitute, with these officers, a Sectional Committee.

The Section shall appoint, from day to day, a Chairman to preside over its meetings.

Rule 7. It shall be the duty of the Sectional Committee of each Section to arrange and direct the proceedings in their Section; to ascertain what communications are offered; to assign the order in which these communications shall appear, and the amount of time which each shall occupy.

The Sectional Committees may likewise recommend subjects for systematic investigation by members willing to undertake the researches, and to present their results at the next meeting.

The Sectional Committee may likewise recommend reports on particular topics and departments of science, to be drawn up as occasion permits, by competent persons, and presented at subsequent meetings.

REPORTS OF PROCEEDINGS.

Rule 8. Whenever practicable the proceedings shall be reported by professional reporters, or stenographers, whose reports are to be revised by the Secretaries before they appear in print.

PAPERS AND COMMUNICATIONS.

Rule 9. No paper shall be placed in the programme, unless admitted by the Sectional Committee; nor shall any be read, unless an abstract of it has been previously presented to the Secretary of the Section, who shall furnish to the Chairman the titles of papers, of which abstracts have been received.

RULE 10. The author of any paper or communication shall be at liberty to retain his right of property therein, provided he declare such to be his wish before presenting it to the Association.

RULE 11. Copies of all communications, made either to the General Association or to the Sections, must be furnished by the authors; otherwise only the titles, or abstracts, shall appear in the published proceedings.

RULE 12. All papers, either at the general or in the sectional meetings, shall be read, as far as practicable, in the order in which they are entered upon the books of the Association; except that those which may be entered by a member of the Standing Committee of the Association shall be liable to postponement by the proper Sectional Committee.

RULE 13. If any communication be not ready at the assigned time, it shall be dropped to the bottom of the list, and shall not be entitled to take precedence of any subsequent communication.

RULE 14. No exchanges shall be made between members without authority of the respective Sectional Committees.

GENERAL AND EVENING MEETINGS.

RULE 15. The Standing Committee shall appoint any general meeting which the objects and interests of the Association may call for, and the evenings shall, as a rule, be reserved for general meetings of the Association.

These general meetings may, when convened for that purpose, give their attention to any topics of science which would otherwise come before the Sections.

It shall be a part of the business of these general meetings to receive the Address of the President of the last meeting; to hear such reports on scientific subjects as, from their general importance and interests, the Standing Committee shall select; also, to receive from the Chairman of the Sections abstracts of the proceedings of their respective Sections; and to listen to communications and lectures explanatory of new and important discoveries and researches in science, and new inventions and processes in the arts.

ORDER OF PROCEEDINGS IN ORGANIZING A MEETING.

Rule 16. The Association shall be called to order by the President of the preceding meeting; and this officer having resigned the chair to the President elect, the General Secretary shall then report the number of papers relating to each department which have been registered, and the Association consider the most eligible distribution into Sections, when it shall proceed to the election of the additional members of the Standing Committee in the manner before described; the meeting shall then adjourn, and the Standing Committee, having divided the Association into sections as directed, shall allot to each its place of meeting for the Session. The Sections shall then organize by electing their officers and their representatives in the Nominating Committee, and shall proceed to business.

PERMANENT SECRETARY.

Rule 17. It shall be the duty of the Permanent Secretary to notify members who are in arrears, to provide the necessary stationery and suitable books for the list of members and titles of papers, minutes of the general and sectional meetings, and for other purposes indicated in the rules, and to execute such other duties as may be directed by the Standing Committee or by the Association.

The Permanent Secretary shall make a report annually to the Standing Committee, at its first meeting, to be laid before the Association, of the business of which he has had charge since its last meeting.

All members are particularly desired to forward to the Permanent Secretary, so as to be received before the day appointed for the Association to convene, complete titles of all the papers which they expect to present during its meeting, with an estimate of the time required for reading each, and such abstracts of their contents as may give a general idea of their nature.

Whenever the Permanent Secretary notices any error of fact or unnecessary repetition, or any other important defect in the papers communicated for publication in the proceedings of the Association, he is authorized to commit the same to the author, or to the proper subcommittee of the Standing Committee for correction.

LOCAL COMMITTEE.

RULE 18. The Local Committee shall be appointed from among members residing at, or near, the place of meeting for the ensuing year; and it shall be the duty of the Local Committee, assisted by the officers, to make arrangements and the necessary announcements for the meeting.

The Secretary of the Local Committee shall issue a circular in regard to the time and place of meetings, and other particulars, at least one month before each meeting.

SUBSCRIPTIONS.

RULE 19. The amount of the subscription, at each meeting, of each member of the Association, shall be two dollars, and one dollar in addition shall entitle him to a copy of the proceedings of the annual meeting. These subscriptions shall be received by the Permanent Secretary, who shall pay them over, after the meeting, to the Treasurer.

The admission fee of new members shall be five dollars, in addition to the annual subscription: and no person shall be considered a member of the Association until this admission fee and the subscription for the meeting at which he is elected have been paid.

RULE 20. The names of all persons two years in arrears for annual dues shall be erased from the list of members; provided that two notices of indebtedness, at an interval of at least three months, shall have been previously given.

ACCOUNTS.

RULE 21. The accounts of the Association shall be audited, annually, by auditors appointed at each meeting.

ALTERATIONS OF THE CONSTITUTION.

RULE 22. No article of this Constitution shall be altered, or amended, or set aside, without the concurrence of three-fourths of the members present, and unless notice of the proposed change shall have been given at the preceding annual meeting.*

* See page xxii.

RESOLUTIONS

OF A PERMANENT AND PROSPECTIVE CHARACTER, ADOPTED AUGUST 19, 1857.

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1. No appointment may be made in behalf of the Association, and no invitation given or accepted, except by vote of the Association or its Standing Committee.

2. The General Secretary shall transmit to the Permanent Secretary for the files, within two weeks after the adjournment of every meeting, a record of the proceedings of the Association and the votes of the Standing Committee. He shall also, daily, during the meetings, provide the Chairman of the two Sectional Committees with lists of the papers assigned to their Sections by the Standing Committee.

3. All printing for the Association shall be superintended by the Permanent Secretary, who is authorized to employ a clerk for that especial purpose.

4. The Permanent Secretary is authorized to put the proceedings of the meeting to press one month after the adjournment of the Association. Papers which have not been received at that time may be published only by title. No notice of articles not approved shall be taken in the published proceedings.

5. The Permanent Chairman of the Sections are to be considered their organs of communication with the Standing Committee.

6. It shall be the duty of the Secretaries of the two Sections to receive copies of the papers read in their Sections, all subsections included, and to furnish them to the Permanent Secretary at the close of the meeting.

7. The Sectional Committees shall meet not later than 9 A.M. daily, during the meetings of the Association, to arrange the programmes of their respective sections, including all sub-

sections, for the following day. No paper shall be placed upon these programmes which shall not have been assigned to the Section by the Standing Committee. The programmes are to be furnished to the Permanent Secretary not later than 11 A.M.

8. During the meetings of the Association, the Standing Committee shall meet daily, Sundays excepted, at 9 A.M., and the Sections be called to order at 10 A.M., unless otherwise ordered. The Standing Committee shall also meet on the evening preceding the first assembling of the Association at each annual meeting, to arrange for the business of the first day; and on this occasion three shall form a quorum.

9. Associate members may be admitted for one, two, or three years as they shall choose at the time of admission,—to be elected in the same way as permanent members, and to pay the same dues. They shall have all the social and scientific privileges of members, without taking part in the business.

10. No member may take part in the organization and business arrangement of both the Sections.

It has been proposed to amend Rule 5 of the Constitution, by inserting before the last section the following: "When not otherwise ordered, the sub-sections shall be as follows:

SECTION A.—1. Mathematics and Astronomy; 2. Physics and Chemistry; 3. Microscopy.

SECTION B.—1. Zoology and Botany; 2. Geology and Palæontology; 3. Ethnology and Archæology."

MEMBERS

OF THE

AMERICAN ASSOCIATION

FOR THE

ADVANCEMENT OF SCIENCE.

A.

Abbe, Cleveland, Cincinnati, Ohio (16).
*Adams, C. B., Amherst, Massachusetts (1).
Adelberg, Justus, New York, New York (15).
Agassiz, Louis, Cambridge, Massachusetts (1).
Aiken, W. E. A., Baltimore, Maryland (12).
Albert, Augustus J., Baltimore, Maryland (12).
Allen, Zachariah, Providence, Rhode Island (1).
Alvord, Benjamin, Omaha, Nebraska (17).
*Ames, M. P., Springfield, Massachusetts (1).
Andrews, Ebenezer, Chicago, Illinois (17).
Andrews, Edmund, Chicago, Illinois (17).
Andrews, E. B., Marietta, Ohio (7).
Andrews, Joseph H., Chicago, Illinois (17).
Angell, James B., Burlington, Vermont (16).
*Appleton, Nathan, Boston, Massachusetts (1).
Atwater, Elizabeth E., Chicago, Illinois (17).
Atwater, Samuel T., Chicago, Illinois (17).

B.

Babcock, Henry H., Chicago, Illinois (17).
*Bache, Alexander D., Washington, District of Columbia (1).
Bacon, John, Jr., Boston, Massachusetts (1).
*Bailey, J. W., West Point, New York (1).
Baird, Lyman, Chicago, Illinois (17).
Baird, S. F., Washington, District of Columbia (1).

NOTE.—Names of deceased members are marked with an asterisk [*]. The figure at the end of each name refers to the meeting at which the elections took place.

Bannister, Henry M., Evanston, Illinois (17).
Bardwell, F. W., Jacksonville, Florida (13).
Barker, G. F., New Haven, Connecticut (13).
Barnard, F. A. P., New York, New York (7).
Barnard, J. G., Washington, District of Columbia (14).
Basnett, Thomas, Ottawa, Illinois (8).
Batchelder, J. M., Cambridge, Massachusetts (8).
Beaty, John F., Chicago, Illinois (17).
*Beck, C. F., Philadelphia, Pennsylvania (1).
*Beck, Lewis C., New Brunswick, New Jersey (1).
*Beck, T. Romeyn, Albany, New York (1).
Beebe, G. D., Chicago, Illinois (17).
*Bell, Samuel N., Manchester, New Hampshire (7).
Benedict, G. W., Burlington, Vermont (16).
Bickmore, Albert S., New York, New York (17).
Bigelow, George H., Burlington, Vermont (16).
Bill, Charles, Chicago, Illinois (17).
*Binney, Amos, Boston, Massachusetts (1).
*Binney, John, Boston, Massachusetts (3).
Blake, Eli W., Ithaca, New York (15).
Blake, Eli W., New Haven, Connecticut (1).
Blake, W. P., San Francisco, California (2).
*Blanding, William, Rhode Island (1).
Blaney, J. Van Zandt, Chicago, Illinois (12).
Blatchford, Eliphalet W., Chicago, Illinois (17).
Bolles, E. C., Brooklyn, New York (17).
Bolton, H. C., New York, New York (17).
*Bomford, George, Washington, District of Columbia (1).
Bouvé, Thomas T., Boston, Massachusetts (1).
Bowditch, Henry I., Boston, Massachusetts (2).
Bowen, Chauncey W., Chicago, Illinois (17).
Bradley, Francis, Chicago, Illinois (17).
Bradley, L., Jersey City, New Jersey (15).
Brevoort, J. Carson, Brooklyn, New York (1).
Briggs, A. D., Springfield, Massachusetts (13).
Briggs, S. A., Chicago, Illinois (17).
Bross, William, Chicago, Illinois (7).
Brown, Robert, Jr., Cincinnati, Ohio (11).
Brush, George J., New Haven, Connecticut (11).
Bryan, Thomas B., Chicago, Illinois (17).
Buchanan, Robert, Cincinnati, Ohio (2).
*Burnap, G. W., Baltimore, Maryland (12).
*Burnett, Waldo I., Boston, Massachusetts (1).
Burroughs, J. C., Chicago, Illinois (17).
Bushee, James, Worcester, Massachusetts (9).
Butler, Thomas B., Norwalk, Connecticut (10).

C.

Calhoun, John B., Chicago, Illinois (17).
Canby, William M., Wilmington, Delaware (17).
*Carpenter, Thornton, Camden, South Carolina (7).
*Carpenter, William M., New Orleans, Louisiana (1).
Carter, Asher, Chicago, Illinois (17)
Case, Leonard, Cleveland, Ohio (15).
Case, L. B., Richmond, Indiana (17).
*Case, William, Cleveland, Ohio (6).
Cassels, J. L., Cleveland, Ohio (7).
Caswell, Alexis, Providence, Rhode Island (2).
Cattell, William C., Easton, Pennsylvania (15).
Chadbourn, P. A., Madison, Wisconsin (10).
Chanute, O., Kansas City, Kansas (17).
Chapman, F. M., Chicago, Illinois (17).
*Chapman, N., Philadelphia, Pennsylvania (1).
Chase, George I., Providence, Rhode Island (1).
*Chase, S., Dartmouth, New Hampshire (2).
Chauvenet, William, St. Louis, Missouri (1).
Chesbrough, E. S., Chicago, Illinois (2).
Chittenden, L. E., New York, New York (14).
*Clapp, Asahel, New Albany, Indiana (1).
Clark, John E., Yellow Springs, Ohio (17).
*Clark, Joseph, Cincinnati, Ohio (5).
*Cleveland, A. B., Cambridge, Massachusetts (2).
Cochran, D. H., Brooklyn, New York (15).
Coffin, James H., Easton, Pennsylvania (1).
Coffin, John H. C., Washington, District of Columbia (1).
Colbert, E., Chicago, Illinois (17).
*Cole, Thomas, Salem, Massachusetts (1).
*Coleman, Henry, Boston, Massachusetts (1).
Conant, Marshall, Northampton, Massachusetts (7).
Conkling, Frederick A., New York, New York (11).
Cope, Edward D., Philadelphia, Pennsylvania (17).
Copes, Joseph S., New Orleans, Louisiana (11).
Corning, Erastus, Albany, New York (6).
Craig, B. F., Washington, District of Columbia (15).
Cramp, J. M., Acadia College, Nova Scotia (11).
Crosby, Alpheus, Salem, Massachusetts (10).
Culver, Howard Z., Chicago, Illinois (17).
Cummings, Joseph, Middletown, Connecticut (13).
Cutting, Hiram A., Lunenburg, Vermont (17).

D.

Dalrymple, E. A., Baltimore, Maryland (11).

Dana, James D., New Haven, Connecticut (1).
Danforth, Edward, Troy, New York (11).
Davis, James, Boston, Massachusetts (1).
Davis, N. S., Chicago, Illinois (17).
Dawson, J. W., Montreal, Canada (10).
*Dean, Amos, Albany, New York (6).
Dean, George W., Fall River, Massachusetts (15).
*Dearborn, George H. A. S., Roxbury, Massachusetts (1).
*Dekay, James E., New York, New York (1).
Delano, B. L., Boston, Massachusetts (16).
Delano, Joseph C., New Bedford, Massachusetts (5).
*Dewey, Chester, Rochester, New York (1).
Dexter, G. M., Boston, Massachusetts (11).
Dinwiddie, Robert, New York, New York (1).
Dixwell, Eps S., Cambridge, Massachusetts (1).
Doggett, Kate N., Chicago, Illinois (17).
Doggett, William E., Chicago, Illinois (17).
Dorr, E. P., Buffalo, New York (15).
Downes, John, Washington, District of Columbia (10).
Drowne, Charles, Troy, New York (6).
*Ducatel, J. T., Baltimore, Maryland (1).
*Dumont, A. H., Newport, Rhode Island (14).
*Duncan, Lucius C., New Orleans, Louisiana (10).
Duncan, T. C., Chicago, Illinois (17).
*Dunn, R. P., Providence, Rhode Island (14).
Dyer, Elisha, Providence, Rhode Island (9).

E.

Easton, Norman, Fall River, Massachusetts (14).
Eaton, Daniel C., New Haven, Connecticut (13).
Eaton, James H., Amherst, Massachusetts (17).
Edwards, J. B., Montreal, Canada (17).
Eimbeck, William, St. Louis, Missouri (17).
Eliot, Charles W., Boston, Massachusetts (14).
Elliott, Ezekiel B., Washington, District of Columbia (10).
Elwyn, Alfred L., Philadelphia, Pennsylvania (1.)
Emerson, George B., Boston, Massachusetts (1).
Englemann, George, St. Louis, Missouri (1).
Engstrom, A. B., Burlington, New Jersey (1).
Eustis, Henry L., Cambridge, Massachusetts (2).
*Everett, Edward, Boston, Massachusetts (2).

F.

Fairbanks, Henry, Hanover, New Hampshire (14).
Farmer, Moses G., Salem, Massachusetts (9).
Farnham, Thomas, Buffalo, New York (15).

Farrar, Henry W., Chicago, Illinois (17).
Ferrell, William, Nashville, Tennessee (11).
Ferris, Isaac, New York, New York (6).
Feuchtwanger, Louis, New York, New York (11).
Fillmore, Millard, Buffalo, New York (7).
Fisher, Davenport, Milwaukie, Wisconsin (17).
Fisher, Mark, Trenton, New Jersey (10).
*Fitch, Alexander, Hartford, Connecticut (1).
Fitch, Edward H., Ashtabula, Ohio (11).
Fitch, O. H., Ashtabula, Ohio (7).
*Forbush, E. B., Buffalo, New York (15).
Foster, Henry, Clifton, New York (17).
Foster, John, Schenectady, New York (17).
Foster, J. W., Chicago, Illinois (1).
*Fox, Charles, Grosse Isle, Michigan (7).
Frothingham, Frederick, Buffalo, New York (11).

G.

*Gay, Martin, Boston, Massachusetts (1).
Gibbon, J. H., Charlotte, North Carolina (3).
Gibbs, Wolcott, Cambridge, Massachusetts (1).
Gill, Theodore, Washington, District of Columbia (17).
*Gillespie, W. M., Schenectady, New York (10).
Gilman, Daniel C., New Haven, Connecticut (10).
*Gilmor, Robert, Baltimore, Maryland (1).
Glynn, James, Geneva, New York (1).
Gold, Theodore S., West Cornwall, Connecticut (4).
*Gould, Augustus A., Boston, Massachusetts (11).
*Gould, B. A., Boston, Massachusetts (2).
Gould, B. A., Cambridge, Massachusetts (2).
*Graham, James D., Washington, District of Columbia (1).
Gray, Asa, Cambridge, Massachusetts (1).
*Gray, James H., Springfield, Massachusetts (6).
Greeley, Samuel S., Chicago, Illinois (17).
Green, Traill, Easton, Pennsylvania (1).
*Greene, Benjamin D., Boston, Massachusetts (1).
Greene, Dascom, Troy, New York (17).
Greene, Francis C., Easthampton, Massachusetts (11).
*Griffith, Robert E., Philadelphia, Pennsylvania (1).
Grimes, J. S., New York, New York (17).
Grinnan, A. G., Orange Court House, Virginia (7).
Grover, Z., Chicago, Illinois (17).
Guyot, Arnold, Princeton, New Jersey (1).

H.

*Hackley, Charles W., New York, New York (4).

Hadley, George, Buffalo, New York (6).
Hager, Hermann A., Cambridge, Massachusetts (17).
Haldeman, S. S., Columbia, Pennsylvania (1).
Hale, Edwin M., Chicago, Illinois (17).
* Hale, Enoch, Boston, Massachusetts (1).
Hall, James, Albany, New York (1).
Hall, N. K., Buffalo, New York (7).
Hamlin, A. C., Bangor, Maine (10).
Hance, Ebenezer, Morrisville, Pennsylvania (7).
Hanover, M. D., Cincinnati, Ohio (13).
* Hare, Robert, Philadelphia, Pennsylvania (11).
* Harlan, Joseph G., Haverford, Pennsylvania (8).
* Harlan, Richard, Philadelphia, Pennsylvania (1).
* Harris, Thaddeus W., Cambridge, Massachusetts (1).
Harrison, B. F., Wallingford, Connecticut (11).
* Hart, Simeon, Farmington, Connecticut (1).
Hartshorne, Henry, Philadelphia, Pennsylvania (12).
Haven, Joseph, Chicago, Illinois (17).
Hawkins, B. W., New York, New York (17).
* Hayden, H. H., Baltimore, Maryland (1).
Hayes, George E., Buffalo, New York (15).
* Hayward, James, Boston, Massachusetts (1).
Henry, Joseph, Washington, District of Columbia (1).
Herzer, W., Columbus, Ohio (15).
Hickcox, S. V. R., Chicago, Illinois (17).
Hickok, W. C., Burlington, Vermont (16).
Hilgard, Eugene W., Oxford, Mississippi (11).
Hilgard, Julius E., Washington, District of Columbia (4).
Hilgard, Theodore C., St. Louis, Missouri (17).
Hill, S. W., Hancock, Lake Superior (6).
Hill, Thomas, Waltham, Massachusetts (8).
Hinrichs, Gustavus, Iowa City, Iowa (17).
Hitt, Isaac R., Chicago, Illinois (17).
Hitchcock, Charles H., Hanover, New Hampshire (11).
* Hitchcock, Edward, Amherst, Massachusetts (1).
Hitchcock, Edward, Amherst, Massachusetts (4).
Hoadley, E. S., Springfield, Massachusetts (13).
Holbrook, J. E., Charleston, South Carolina (1).
Holmes, E. L., Chicago, Illinois (17).
Holmes, Henry A., Albany, New York (11).
Horsford, E. N., Cambridge, Massachusetts (1).
* Horton, William, Craigville, New York (1).
Hough, Franklin B., Lowville, New York (4).
Hough, G. W., Albany, New York (15).
* Houghton, Douglas, Detroit, Michigan (1).
Howell, Robert, Nichols, New York (6).

Hoy, Philo R., Racine, Wisconsin (17).
Hubbard, Gurdon S., Chicago, Illinois (17).
Hubbard, Oliver P., Hanover, New Hampshire (1).
Hubbard, Sara A., Kalamazoo, Michigan (17).
*Hubbert, James, Richmond, Province of Quebec (16).
Hungerford, Edward, Burlington, Vermont (10).
Hunt, Charles S., New York, New York (17).
*Hunt, E. B., Washington, District of Columbia (2).
*Hunt, Freeman, New York, New York (11).
Hunt, George, Providence, Rhode Island (9).
Hunt, T. Sterry, Montreal, Canada (1).
Hyatt, James, Bangall, New York (10).

I.

*Ives, Thomas P., Providence, Rhode Island (10).

J.

Jenks, J. W. P., Middleboro', Massachusetts (2).
Jillson, B. C., Nashville, Tennessee (14).
*Johnson, W. R., Washington, District of Columbia (1).
Johnston, John, Middletown, Connecticut (1).
*Jones, Catesby A. R., Washington, District of Columbia (8).
Joy, C. A., New York, New York (8).
Judd, Orange, New York, New York (4).

K.

Kedzie, J. H., Chicago, Illinois (17).
Keely, G. W., Waterville, Maine (1).
Keep, N. C., Boston, Massachusetts (13).
Kerr, W. C., Raleigh, North Carolina (10).
Kimball, J. P., New York, New York (15).
King, Mary B. A., Rochester, New York (15).
Kirkpatrick, James A., Philadelphia, Pennsylvania (7).
Kirkwood, Daniel, Canonsburg, Pennsylvania (7).
Kite, Thomas, Cincinnati, Ohio (5).
Klippart, John H., Columbus, Ohio (17).
Knickerbocker, Charles, Chicago, Illinois (17).

L.

Lapham, Increase A., Milwaukee, Wisconsin (3).
*Lasel, Edward, Williamstown, Massachusetts (1).
Lattimore, S. A., Rochester, New York (15).
Lawrence, George N., New York, New York (7).
Lea, Isaac, Philadelphia, Pennsylvania (1).
Leakin, George A., Baltimore, Maryland (17).
LeConte, John L., Philadelphia, Pennsylvania (1).

*Lederer, Baron von, Washington, District of Columbia (1).
Lesley, Joseph, Jr., Philadelphia, Pennsylvania (8).
Lesley, J. P., Philadelphia, Pennsylvania (2).
Letchworth, William P., Portage, New York (15).
*Lieber, Oscar M., Columbia, South Carolina (8).
*Lincklaen, Ledyard, Cazenovia, New York (1)
Lincoln, Robert T., Chicago, Illinois (17).
Lindsley, J. B., Nashville, Tennessee (1).
*Linsley, James H., Stafford, Connecticut (1).
Little, George, Oxford, Mississippi (15).
Locke, Luther F., Nashua, New Hampshire (7).
Logan, William E., Montreal, Canada (1).
Lombard, Benjamin, Chicago, Illinois (17).
Loomis, Elias, New Haven, Connecticut (1).
Loosey, Charles F., New York, New York (12).
*Lothrop, Joshua R., Buffalo, New York (15).
Lovering, Joseph, Cambridge, Massachusetts (2).
Lunn, William, Montreal, Canada (11).
Lupton, N. T., Greensboro, Alabama (17).
Lyman, B. S., Philadelphia, Pennsylvania (15).
Lyman, Chester S., New Haven, Connecticut (14).
Lyman, Henry M., Chicago, Illinois (17).
Lynch, P. N., Charleston, South Carolina (2).

M.

*M'Conihe, Isaac, Troy, New York (4).
McCagg, Ezra B., Chicago, Illinois (17).
McMurtrie, Horace, Boston, Massachusetts (17).
McRae, John, Camden, South Carolina (3).
Marcy, Oliver, Evanston, Illinois (10).
*Marsh, Dexter, Greenfield, Massachusetts (1).
Marsh, H. H., Chicago, Illinois (17).
Marsh, O. C., New Haven, Connecticut (15).
Marshall, Charles D., Buffalo, New York (15).
Marshall, Orasmus H., Buffalo, New York (15).
*Mather, William W., Columbus, Ohio (1).
Mauran, J., Providence, Rhode Island (2).
Mayhew, D. P., Ypsilanti, Michigan (13).
Maynard, Alleyne, Cleveland, Ohio (7).
Meade, George G., Philadelphia, Pennsylvania (15).
Means, A., Oxford, Georgia (5).
Meehan, Thomas, Germantown, Pennsylvania (17).
Meek, F. B., Washington, District of Columbia (6).
Meigs, James A., Philadelphia, Pennsylvania (12)
Miles, Henry H., Quebec, Canada East (11).
Miller, Samuel, New Haven, Connecticut (14).

Minifie, William, Baltimore, Maryland (12).
Mitchell, Maria, Poughkeepsie, New York (4).
Mitchell, William H., Florence, Alabama (17).
Morgan, Lewis H., Rochester, New York (10).
Morris, John G., Baltimore, Maryland (12).
* Morton, S. G., Philadelphia, Pennsylvania (1).
Murray, David, New Brunswick, New Jersey (11).

N.

Nason, Henry B., Troy, New York (13).
Nelson, Cleland K., Annapolis, Maryland (12).
Newberry, J. S., New York, New York (5).
Newcomb, Simon, Washington, District of Columbia (13).
* Newton, E. H., Cambridge, New York (1).
Newton, Hubert A., New Haven, Connecticut (6).
Nichols, Charles A., Providence, Rhode Island (17).
* Nicollett, J. N., Washington, District of Columbia (1).
Niles, W. H., Cambridge, Massachusetts (16).
* Norton, J. P., New Haven, Connecticut (1).
Norton, W. A., New Haven, Connecticut (6).

O.

* Oakes, William, Ipswich, Massachusetts (1).
Ogden, Mahlon D., Chicago, Illinois (17).
Ogden, W. B., Chicago, Illinois (17).
Oliver, James Edward, New York, New York (7).
* Olmsted, Alexander F., New Haven, Connecticut (4).
* Olmsted, Denison, New Haven, Connecticut (1).
* Olmsted, Denison, Jr., New Haven, Connecticut (1).
Ordway, John M., Jamaica Plain, Massachusetts (9).
Osten Sacken, Baron R. von, New York, New York (10).

P.

Packard, A. S., Jr., Salem, Massachusetts (16).
Page, Peter, Chicago, Illinois (17).
Paine, Cyrus F., Rochester, New York (12).
Painter, Minshall, Lima, Pennsylvania (7).
* Parkman, Samuel, Boston, Massachusetts (1).
Parmelee, Dubois D., New York, New York (15).
Parry, Charles C., Washington, District of Columbia (6).
Peabody, S. H., Chicago, Illinois (17).
Peck, Edward W., Burlington, Vermont (16).
Peirce, Benjamin, Cambridge, Massachusetts (1).
Perkins, George H., Providence, Rhode Island (17).
Perkins, George R., Utica, New York (1).
Perkins, Maurice, Schenectady, New York (15).
Perry, John B., Cambridge, Massachusetts (16).

*Perry, M. C., New York, New York (10).
Phelps, Almira L., Baltimore, Maryland (13).
Phelps, Charles E., Baltimore, Maryland (13).
*Plumb, Ovid, Salisbury, Connecticut (9).
*Porter, John A., New Haven, Connecticut (14).
Pourtales, L. F., Washington, District of Columbia (1).
Powell, Edwin, Chicago, Illinois (17).
Prescott, William, Concord, New Hampshire (1).
Pruyn, J. V. L., Albany, New York (1).
*Pugh, Evan, Centre Co., Pennsylvania (14).
Pumpelly, Raphael, Cambridge, Massachusetts (17).
Putnam, F. W., Salem, Massachusetts (10).

Q.

Quincy, Edmund, Jr., Boston, Massachusetts (11).

R.

Rauch, J. H., Chicago, Illinois (11).
Raymond, R. W., New York, New York (15).
Read, Daniel, Columbia, Missouri (11).
Redfield, John H., Philadelphia, Pennsylvania (1).
*Redfield, William C., New York, New York (1).
Riley, Charles V., St. Louis, Missouri (17).
Ritchie, E. S., Boston, Massachusetts (10).
Robertson, Thomas D., Rockford, Illinois (10).
Rochester, Thomas F., Buffalo, New York (15).
Rockwell, Alfred P., New Haven, Connecticut (10).
Rockwell, John, La Salle, Illinois (11).
*Rockwell, John A., Norwich, Connecticut (10).
Rockwell, Joseph P., New Haven, Connecticut (17).
Rodman, William M., Providence, Rhode Island (9).
Rogers, Fairman, Philadelphia, Pennsylvania (11).
*Rogers, James B., Philadelphia, Pennsylvania (1).
Rogers, W. A., Alfred Centre, New York (15).
Rogers, W. B., Boston, Massachusetts (1).
Rood, O. N., New York, New York (14).
Roosevelt, Clinton, New York, New York (11).
Root, Edward W., Clinton, New York (17).
Rumsey, Bronson C., Buffalo, New York (15).
Rumsey, George T., Chicago, Illinois (17).
Runkle, J. D., Boston, Massachusetts (2).
Rutherford, Louis M., New York, New York (13).
Ryerson, Joseph T.. Chicago, Illinois (17).

S.

Safford, J. M., Nashville, Tennessee (6).
Safford, Truman, H., Chicago, Illinois (13).

Sanborn, Francis G., Boston, Massachusetts (13).
Sargent, Rufus, Auburn, New York (10).
Scammon, J. Young, Chicago, Illinois (17).
Schanck, J. Stillwell, Princeton, New Jersey (4).
Schott, Charles A., Washington, District of Columbia (8).
Scudder, Samuel H., Cambridge, Massachusetts (13).
Seward, William H., Auburn, New York (1).
Sheafer, P. W., Pottsville, Pennsylvania (4).
Sheldon, Edwin H., Chicago, Illinois (17).
Shepard, C. U., Amherst, Massachusetts (4).
Sherwood, Albert, Buffalo, New York (15).
Sias, Solomon, Charlotteville, New York (10).
Sill, Elisha N., Cuyahoga Falls, Ohio (6).
*Silliman, Benjamin, New Haven, Connecticut (1).
Silliman, Benjamin, New Haven, Connecticut (1).
Smith, A. D., Providence, Rhode Island (14).
Smith, J. L., St. Louis, Missouri (14).
*Smith, J. V., Cincinnati, Ohio (5).
Smith, James Y., Providence, Rhode Island (9).
*Smith, Lyndon A., Newark, New Jersey (9).
Snell, Eben S., Amherst, Massachusetts (2).
*Sparks, Jared, Cambridge, Massachusetts (2).
Spencer, Charles A., Brooklyn, New York (14).
Sprague, Albert A., Chicago, Illinois (17).
Spring, Charles H., Boston, Massachusetts (13).
Stanard, Benjamin A., Cleveland, Ohio (6).
Stearns, Josiah A. ,Boston, Massachusetts (10).
Steiner, Lewis H., Frederick City, Maryland (7).
Stimpson, William, Chicago, Illinois (12).
Stoddard, O. N., Oxford, Ohio (7).
Stone, Samuel, Chicago, Illinois (17).
Storer, D. H., Boston, Massachusetts (1).
Storer, Frank H., Boston, Massachusetts (13).
Sullivant, W. S., Columbus, Ohio (7).
Swasey, Oscar F., Beverly, Massachusetts (17).

T.

*Tallmadge, James, New York, New York (1).
*Taylor, Richard C., Philadelphia, Pennsylvania (1).
Tenney, Samuel, Williamstown, Massachusetts (17).
*Teschemacher, J. E., Boston, Massachusetts (1).
Thompson, Aaron R., New York, New York (1).
Thompson, Harvey M., Chicago, Illinois (17).
*Thompson, Z., Burlington, Vermont (1).
*Thurber, Isaac, Providence, Rhode Island (9).
Tillman, S. D., New York, New York (15).

Tingley, Jeremiah, Meadville, Pennsylvania (15).
Tingley, Joseph, Greencastle, Indiana (14).
Tolles, Robert B., Boston, Massachusetts (15).
Torrey, John, New York, New York (1).
*Totten, J. G., Washington, District of Columbia (1),
Townsend, Franklin, Albany, New York (4).
*Townsend, John K., Philadelphia, Pennsylvania (1).
Townshend, N. S., Avon, Ohio (17).
Tracy, John F., Chicago, Illinois (17).
Trembly, J. B., Toledo, Ohio (17).
*Troost, Gerard, Nashville, Tennessee (1).
True, Nathaniel T., Bethel, Maine (17).
*Tuomey, M., Tuscaloosa, Alabama (1).
Tuttle, Albert H., Cleveland, Ohio (17).
*Tyler, Edward R., New Haven, Connecticut (1).
Tyson, Philip T., Baltimore, Maryland (12).

U.

Upham, J. Baxter, Boston, Massachusetts (14).
Upton, George P., Chicago, Illinois (17).

V.

*Vancleve, John W., Dayton, Ohio (1)
Van der Weyde, P. H., New York, New York (17).
Van Duzee, Mary K., Buffalo, New York (16).
Van Pelt, William, Williamsville, New York (7).
*Vanuxem, Lardner, Bristol, Pennsylvania (1).
Vaux, William S., Philadelphia, Pennsylvania (1).
Verrill, A. E., New Haven, Connecticut (16).
Vose, George L., Paris Hill, Maine (15).
Vought, John H., Buffalo, New York (15).

W.

Waddell, John N., Oxford, Mississippi (17).
*Wadsworth, James S., Genesee, New York (2).
*Wagner, Tobias, Philadelphia, Pennsylvania (9).
Wales, Torrey E., Burlington, Vermont (16).
Wales, William, Fort Lee, New York (15).
Walker, George C., Chicago, Illinois (17).
*Walker, Joseph, Oxford, New York (10).
*Walker, Sears C., Washington, District of Columbia (1).
*Walker, Timothy, Cincinnati, Ohio (4).
Walling, H. F., Easton, Pennsylvania (16).
Ward, Henry A., Rochester, New York (13).
Ward, R. H., Troy, New York (17).
Warren, G. K., Washington, District of Columbia (12).

*Warren, John C., Boston, Massachusetts (1).
Watson, James C., Ann Arbor, Michigan (13).
Watson, William, Cambridge, Massachusetts (12).
*Webster, H. B., Albany, New York (1).
*Webster, J. W., Cambridge, Massachusetts (1).
*Webster, M. H., Albany, New York (1).
Webster, Nathan B., Kenansville, North Carolina (7).
Wenz, J., New Orleans, Louisiana (15).
Wheatland, Henry, Salem, Massachusetts (1).
*Wheatland, Richard H., Salem, Massachusetts (13).
Wheatley, Charles M., Phœnixville, Pennsylvania (1).
Wheeler, T. B., Montreal, Canada (11).
Wheildon, W. W., Charlestown, Massachusetts (13).
Whitney, Asa, Philadelphia, Pennsylvania (1).
Whitney, William D., New Haven, Connecticut (12).
Whittlesey, Charles, Cleveland, Ohio (1).
*Willard, Emma, Troy, New York (15).
Williams, Henry W., Boston, Massachusetts (11).
Williamson, R. S., San Francisco, California (12).
Wilson, Charles L., Chicago, Illinois (17).
Winchell, Alexander, Ann Arbor, Michigan (3).
Winslow, Ferdinand S., Chicago, Illinois (17).
*Woodbury, L., Portsmonth, New Hampshire (1).
Woodworth, John M., Chicago, Illinois, (17).
Worthen, A. H., Springfield, Illinois (5).
Wright, A. W., Williamstown, Massachusetts (14).
Wright, Chauncey, Cambridge, Massachusetts (9).
*Wright, John, Troy, New York (1).
Wurtele, Louis C., Acton Vale, Canada East (11).
Wurtz, Henry, New York, New York (10).
Wyman, Jeffries, Cambridge, Massachusetts (1).

Y.

Youmans, E. L., New York, New York (6).
*Young, Ira, Hanover, New Hampshire (7).

This list contains five hundred and twenty names, of which one hundred and nineteen are of deceased members. The names of those who were chosen at Salem, and who have already joined the Association, have not yet been incorporated into the general catalogue of members, but are printed separately on the following pages.

MEMBERS

WHO JOINED AT

THE SALEM MEETING.

A.

Adams, Edwin F., Charlestown, Massachusetts.
Adams, Samuel, Jacksonville, Illinois.
Agassiz, Alexander E. R., Cambridge, Massachusetts.
Allen, J., Alfred Centre, New York.
Allen, Joel A., Cambridge, Massachusetts.
Austin, E. L., Plymouth, Ohio.
Austin, E. P., Cambridge, Massachusetts.

B.

Bachelder, J. H., Salem, Massachusetts.
Bailey, Loring W., Frederickton, New Brunswick.
Bailey, W. W., Providence, Rhode Island.
Barnard, James M., Boston, Massachusetts.
Bethune, Charles J. S., Credit, Canada.
Bicknell, Edwin, Salem, Massachusetts.
Bliss, Porter C., New York, New York.
Boynton, John F., Syracuse, New York.
Briggs, D. H., Norton, Massachusetts.
Bryan, Oliver N., Baltimore, Maryland.
Burbank, L. S., Lowell, Massachusetts.

C.

Chase, Pliny E., Philadelphia, Pennsylvania.
Chase, R. Stuart, Haverhill, Massachusetts.
Clarke, F. W., Ithaca, New York.
Cogswell, William, Salem, Massachusetts.
Cogswell, George, Bradford, Massachusetts.
Cook, George H., New Brunswick, New Jersey.
Cooke, C., Salem, Massachusetts.
Crampton, R. C., Jacksonville, Illinois.
Crosby, Thomas R., Hanover, New Hampshire.

Cummings, John, Woburn, Massachusetts.
Curtis, Josiah, Boston, Massachusetts.

D.

Dall, William H., Washington, District of Columbia.
De Laski, John, Falmouth, Maine.
Devereux, J. H., Cleveland, Ohio.

E.

Edwards, A. M., New York, New York.
Ellenwood, Charles N., San Francisco, California.
Emerton, James H., Salem, Massachusetts.
Endicott, William C., Salem, Massachusetts.

F.

Fellows, R. S., New Haven, Connecticut.
Fenton, William, Troy, New York.
Foucou, Felix, Madison, Wisconsin.
Frothingham, Richard, Charlestown, Massachusetts.

G.

Goessman, C. A., Amherst, Massachusetts.
Goodell, Abner C., Jr., Salem, Massachusetts.
Gregory, J. J. H., Marblehead, Massachusetts.

H.

Hagar, D. B., Salem, Massachusetts.
Hall, L. B., Hanover, New Hampshire.
Hambly, J. B., Portsmouth, Rhode Island.
Hamel, Thomas E., Quebec, Canada.
Hartt, Charles F., Ithaca, New York.
Hoyt, J. W., Madison, Wisconsin.
Humphrey, D., Lawrence, Massachusetts.
Hyatt, Alpheus, Salem, Massachusetts.

J.

Jasper, G. A., Charlestown, Massachusetts.
Johnson, Amos H., Salem, Massachusetts.

L.

Lambert, Thomas R., Charlestown, Massachusetts.
Langley, S. S., Pittsburg, Pennsylvania.
Lawrence, Edward, Charlestown, Massachusetts.
Lockwood, Samuel, Keyport, New Jersey.
Loring, George B., Salem, Massachusetts.
Lyon, Henry, Charlestown, Massachusetts.

M.

Maack, G. A., Cambridge, Massachusetts.
Mack, David, Belmont, Massachusetts.
McNeil, J. A., Grand Rapids, Michigan.
Marden, George H., Charlestown, Massachusetts.
Maynard, George W., Troy, New York.
Monroe, William, Boston, Massachusetts.
Morley, Edward W., Pittsfield, Massachusetts.
Morse, Edward S., Salem, Massachusetts.

N.

Nichols, William R., Boston, Massachusetts.

O.

Orton, James, Poughkeepsie, New York.

P.

Paine, Nathaniel, Worcester, Massachusetts.
Patton, William W., Chicago, Illinois.
Peckham, S. F., Providence, Rhode Island.
Peirce, B. O., Beverly, Massachusetts.
Perkins, Henry C., Newburyport, Massachusetts.
Phippen, George D., Salem, Massachusetts.
Pickering, Edward C., Boston, Massachusetts.

R.

Rice, William N., Middletown, Connecticut.
Rogers, Robert E., Philadelphia, Pennsylvania.
Rossetes, G. R., Marietta, Ohio.

S.

Samson, George W., Washington, District of Columbia.
Scofield, Samuel L., New York, New York.
Seely, Charles A., New York, New York.
Shepard, L. D., Boston, Massachusetts.
Sherwood, Andrew, Mansfield, Pennsylvania.
Smith, Isaac T., New York, New York.
Smith, Rollin A., Fond-du-Lac, Wisconsin.
Smith, Sidney I., New Haven, Connecticut.
Squier, E. G., New York, New York.
Stearns, R. E. C., San Francisco, California.
Stephens, W. H.. Lowville, New York.
Stevens, R. P., New York, New York.
Stimpson, Frederick E., Boston, Massachusetts.
Stimpson, T. M., Peabody, Massachusetts.
Stoughton, T. M., Gill, Massachusetts.

T.

Turner, R. S., Reading, Pennsylvania.
Twining, A. C., New Haven, Connecticut.

U.

Utley, Charles H., Buffalo, New York.

V.

Vail, Hugh D., Philadelphia, Pennsylvania.
Valentine, Benjamin E., Brooklyn, New York.

W.

Walker, Charles A., Chelsea, Massachusetts.
Wanzer, Ira, Lanesville, Connecticut.
Warner, James D., Brooklyn, New York.
Warren, G. W., Charlestown, Massachusetts.
Webb, Benjamin, Jr., Salem, Massachusetts.
Wells, Daniel H., New Haven, Connecticut.
White, A. D., Ithaca, New York.
Whitfield, R. P., Albany, New York.
Williams, H. S., New Haven, Connecticut.
Woolworth, S. B., Albany, New York.

Y.

Young, Charles A., Hanover, New Hampshire.

ADDRESS

OF

BENJAMIN APTHORP GOULD,

EX-PRESIDENT OF THE ASSOCIATION.

Mr. President and Gentlemen of the American Association for the Advancement of Science.

The usage, and even the fundamental law, of this Association entail upon its retiring President a duty which he may not evade: that of delivering at the meeting following his term of official service, a formal address. Although the fulfilment of this duty is but a very inadequate return for the honor which your partiality has conferred upon him, still it is not without its embarrassments, not the least of which is the apprehension of comparison with the utterances on previous occasions by great and honored philosophers who have presided over your meetings. Many of their words were spoken for the children's children of those who listened. Reaching the very souls of those who heard at the moment, they became there endowed with new life and energy, each recipient becoming a center of emanation for what in them was true and important; while they survive upon the printed page for transmission in letter as well as spirit to unborn generations.

Would that I could offer such precious thoughts or incentives for your acceptance. How can I presume to speak as a successor of Bache, Henry, Agassiz and the other great investigators to whom in turn you have confided the guidance of the Association. Yet as their follower, in another sense, I may address you, for I believe that the few ideas which I propose laying before you would find with them approval and indorsement.

With your permission I will speak of the position of the scientific investigator in the community; of the duties incumbent upon him, and of what he may rightfully expect in return, under the present relations of civilized and educated communities; of the peculiar opportunities and restrictions in the United States today; of the obstacles which beset the path of the laborer in scientific fields; and of the results which he may be justified in anticipating as the reward of patient, unfaltering, conscientious effort in his high vocation.

We are accustomed to regard ourselves as belonging to a new country, and to palliate in our own hearts, even if not openly, our intellectual short-comings and our deficiencies in learning or culture, by this plea. And just as men who have attained eminence, notwithstanding an absence of early opportunities, are often accustomed to glory in this want of advantages, as though it had been a merit rather than a misfortune,—judging themselves always by a relative, instead of an absolute, standard,—so we American lovers of science are too much inclined to take note of the difficulties against which we have struggled or are struggling, rather than of the actual level which we have attained by the effort. Of course there is much to extenuate in this proclivity; how much, you all know. The want, until a recent date, of books of reference; the want of access to such implements of research as are beyond the reach of most private men; the want of time and energy to spare from the grand "struggle for existence;" and above all, the want of competent scientific counselers and guides for the beginner in scientific research; all these are well known to those of you who have attained an age at all approaching the meridian of life. Before the omniscient Judge, they will surely be credited to each individual in the great account. But if we consider not the individuals but the people, and hold the community responsible for its collective failures, while we give it credit for its collective achievements, in the intellectual field, I am sometimes apprehensive that we are given to pluming ourselves too much, and to estimating our progress rather by the number of obstacles which we know to have been surmounted, than by the mile-stones which have been left behind. Communities have merits and failings, as individuals have;

they are but the integral of their many constituent individuals; and among our national failings can scarcely be counted that of judging ourselves too harshly. Our aim should not be to overcome difficulties, except so far as this is a necessary means of advance; it is progress, toward which our efforts should be directed, and if the obstacles are serious, we have, as a people, no right to credit merely for having surmounted them, provided we possess, and do not employ, the power to remove them. Can the intellectual standing and rank of a nation be fairly measured by the highest achievements of its ablest and most devoted men, if so be that these men or their deeds are not the legitimate fruit of the tendencies and influences at work, but, on the contrary, are exceptional cases, which have maintained their existence and even blossomed out by virtue of the humanity that was in them, notwithstanding hindrances and discouragements?

Two hundred and forty years have passed since our ancestors sought the wooded hills, the sheltered valleys, and broad meadow lands which skirt the coast of Massachusetts Bay, and amid which we are assembled here. Less than forty miles from this spot, landed the stern pilgrims of Plymouth, eight years earlier. Ten or twelve years earlier still had been founded the English settlement at Jamestown; but it has been the established verdict of history that from the region where we now are, from within a circle not thirty miles in radius around yonder metropolis, may the spread of arts, letters and science be traced throughout this broad land. Within six years from the arrival of the first settlers of the Massachusetts Bay, they voted to establish a college, and appropriated therefore a sum "equal to a year's rate of the whole colony." In the ensuing year they changed the name of the town in which they placed it from Newton to Cambridge, "a grateful tribute to the trans-Atlantic literary parent of many of the first emigrants, and indicative of the high destiny to which they intended the institution they were establishing should aspire." A single year later, John Harvard, a graduate of Emanuel College, Cambridge, bequeathed to the incipient university his library and one-half his fortune, amounting to nearly twice what the Colony had voted. "The example of Harvard," says President Quincy, "was like an electric spark, falling upon materials of a sympathetic nature, exciting immediate action and consentaneous energy. The magistrates caught the spirit and led the way by a subscription among themselves of two hundred pounds, in books for the library. The comparatively wealthy followed with gifts of twenty and thirty pounds. The needy multitude succeeded like the widow of old, 'casting their mites into the treasury.'"

These facts I recall to your memories, to show the length of time during which our national culture has been receiving shape and character. For two centuries and a third, at least, the characteristic, mental and intellectual tendencies of this people have been forming themselves. Our fathers brought with them such culture as the best seminaries of their native land could give; they represented the best intellect of the several classes of the mother country. The professional, the titled, the mechanic and the agricultural classes of New-England were severally of an intellect and culture much above the average of the same classes in old England, whence they came. The standards of scholarship, of science and of art, which they brought across the seas, were the same which they left behind. For the intellectual progress of the world since then, this community has owed its proportionate share, to be reckoned in the joint ratio of its population and of its initial advantages.

True, our people has had the forest, the desert and the red man to vanquish; it has had the social problems to solve, first of a protestant hierarchy, then of independence of all priestcraft, kingcraft and feudalism, and finally of equality before the law, for all who wear the form of man and are created in the image of God. True, it has had its share of trials from foes without and traitors within, and has had the strong bonds of hereditary political and intellectual dependence to burst. True, it has already given to the world many a masterwork, in the arts of peace and the arts of war; the steamboat, the cotton-gin and the sewing machine; the practical application of the electric telegraph, and the means of its printed record; the most perfect forms yet attained for the steam-engine and the steam-boiler; the most powerful ordnance and the most impregnable vessels; the telescopes of Clark and Fitz, the microscopes of Spencer and Tolles, and the means of annihilating pain. True, it has planted the starry flag upon the Antarctic continent and by the Polar sea, and has given to history many a name of the wise and good, whose blessed memory can never be hemmed in by oceans. Heaven forbid that any son of America should shut his eyes to these subjects of honest pride, or to a host of others like them, which I have no need to recount! If love of country be a virtue, assuredly it is not a difficult one for us to exercise. But what I would now say is, that, whatever may be the claims of our country to have done her part in the furtherance of civilization so far as depends upon the solution of high political problems and upon advancement in the arts, her contributions to science have not kept pace with these; nor indeed with those of several European nations, which have had to contend against obstacles quite comparable in magnitude with our own, even though of a totally different nature. France, torn asunder by frenzied convulsions and internal throes, such as no other civilized nation has ever been called on to endure,—Germany, trampled under foot again and again by foreign invaders, civil strife and domestic oppression,—Russia, lately emerged from Asiatic barbarism and contending at once against the Turk, the Tartar and the western foe,—have they not had their share of hindrances to scientific progress, great even if inferior to those offered by the forest and the savage? Yet it would ill beseem us to invite a comparison with them in any department of science, physical or natural. Equate out the names of a very few men on each side, wherever this seem possible, and what an overwhelming preponderance would then throw the Western scale aloft.

"Two hundred and forty years," I hear some one say, "what are they in the development of a nation, or of its scientific character? Twenty-five centuries have passed since Thales predicted an eclipse of the sun; nineteen, since Sosigenes reformed the calendar for Julius Cæsar; fourteen hundred years have rolled over the University of Bologna. What to you occidentals seems a hoary antiquity, is a mere yesterday for the dweller by the Tiber, the Thames, the Seine, the Danube or the

Rhine." Be it so! Yet Hans Lippersheim's first suggestion of a telescope was eighteen months after Newport had sailed up the James river with his infant colony. The idea of a logarithm was then not born: Napier and Briggs were names unknown to fame. The oaks and beeches had been cleared from these hills, and our ancestors had built their rustic homes, at the time when Galileo was tortured into abjuring the profane doctrine that the earth moved, and not the sun. When Harvard endowed the college that bears his name, there was no such thing as a barometer or a thermometer. It is within these very two hundred and forty years that modern science has come into existence, and the world's intellect been turned from speculation to investigation. It is within this period that our implements of research have been devised, that the air-pump, the electrical machine and the clock have been invented, that every public chemical laboratory, every astronomical or physical observatory, and every academy of sciences has been founded. Boston had been settled when Keppler died. The grandchildren of the original colonists of Plymouth and the Massachusetts Bay were born, when the law of universal gravitation was first proclaimed by Newton.

Therefore it is that we must confess our scientific progress to have been far inferior to that of several European nations. And I fear that the confession might truthfully be made much broader, and include our progress in all purely intellectual studies, which hold forth no promise of immediate utility in promoting physical well-being or material convenience. If this is true, my friends, it is time that it should be so no longer. And before you, the declared lovers of science,—in this Association formed to promote her welfare and advancement,—here in the earliest seat of that colony, whence has geographically radiated what of culture and of science our country has possessed,—I would fain say some few words which, however crude or ill-arranged, might find a congenial soil within your hearts—to bear fruit, perhaps, when all of us have disappeared from the stage—and which might aid, in however small a degree, to avert the day when the highest recognized aims shall be toward material prosperity, rather than toward intellectual development and progress.

There must be in every community men specially endowed with scientific tastes and impulses. In most cases such innate tendencies accompany especial gifts in the same direction, and although in the infinitely varied scheme of nature, this is not always the case, yet the exceptions are few, and the incentives to exertion, which such tastes supply, do much toward atoning for the lack of original power. Wherever positions of honor or emolument are available for the man of science, these become objects of ambition or of greed to another class of men who aspire to them as ends to be attained and not as means of scientific progress. It is to such that Schiller referred when he said of science,

> "Einem ist sie die hohe, die himmlische Göttin; dem andern
> Eine tüchtige Kuh, die ihn mit Butter versorgt."*

Between these two classes it is impossible to draw a sharply defined line. They shade into one another by such imperceptible gradations, that many a man might be unable, in his strictest communings with himself, to decide as to which he himself belonged. Then there is an intermediate class, whom circumstance guides into the scientific path and who are endowed with a versatility which enables them effectively to follow out any career to which they earnestly devote themselves.

Now the social problem here evidently is, so to order the influences and attune the public sentiment in the community as to allow the ablest minds to labor in those fields for which they are best adapted, and to guide the most versatile, so far as possible, into such channels that their energies may promote the highest welfare of society.

The magnitude of the class of scientific men in any community is clearly dependent, to a very great extent, upon the intellectual condition of that community. Probably no civilized society, totally devoid of a scientific element, ever did or ever will exist. In ancient Rome its amount appears to have been a minimum, yet in ancient Greece it was far otherwise; whence we may infer that the fine arts and *belles-lettres* are in themselves neither conducive nor antagonistic to science, in any marked degree. At all times and places there have been

*"She is the high, the heavenly Goddess to one; to another
But a convenient cow, that gives him his butter and cheese."

some in whom the divine fire burned; and so it doubtless ever will be. Such could no more be turned from their high instinct to discover causes and laws, than the mountain torrent from its course toward the sea. Yet how few are these, although they have never failed to pass the torch from age to age! Even in the days of Roman dominion, Africa nursed the embers of the sacred flame; and swarthy Arabs and Moors, with here and there a silent monk, guarded it through the dark ages; ages replete with classic lore, with wondrous art, with barbaric luxury, yet devoid of science, except in the secret guardianship of those who dared not betray their priceless yet mysterious possession.

To such men the civilization of today permits freedom of inquiry and of utterance, at least, and awards a certain modicum of public recognition and respect, limited, it is true, not by the good will of the community, but by its means of appreciating the character and scope of their labors. These are the men, nevertheless, to whom indirectly the world owes its material progress, although the intermediate steps, between their researches and the ingenious inventions by which their results are practically utilized, are rarely traced, even by those who reap the harvest. Yet it is not for the sake of material progress that they have toiled; this is simply the world's recompense for having harbored them. *Sic vos non vobis mellificatis, aves.* These are the men who toil on in their lofty studies, seeking the truth for its own sake, drawn as by some resistless magnetism, and working even better than they know. Poverty cannot suppress the instinct; ridicule cannot prevent its exercise; persecution cannot deter from the utterance of its results.

This scanty class constitutes the minimum number of the followers of science for any community. The additional number is greater or less according to the amount of personal sacrifice requisite for following the inborn impulse — since the intensity of this impulse varies in every degree, — or according to the temptations offered for joining the ranks of those who adopt science as a business. It is easy to see that there is danger to the intellectual progress of the community at each extreme. Where the votary of science must sacrifice all to follow her, her welfare is scarcely more imperiled than where the guardianship of her interests, and the means of extending her domain, are confided to the hands of those who would make of her a servant, and not a sovereign.

By an unhappy, though perhaps natural, mischance, the English language has had no name for the scientific investigator, nor word to denote his calling. There is no nobler word than philosopher, — lover of knowledge; yet, in the score of centuries since this grand old word has been in use, its meaning, if not perverted, has at least been narrowed and distorted. The French expression, *savant*, has sometimes been pressed into service by those who have felt the want of some appropriate term; but, without undertaking to criticise the aptness of this word, it is most certain that the time has come when our own language demands some name for the class of men who give their lives to scientific study. Therefore it was, that twenty years ago I ventured to propose one, which has been slowly finding its way to general adoption; and the word *scientist*, though scarcely euphonious, has gradually assumed its place in our vocabulary. Philologically, it is subject to criticism, as being from a Latin root with a Greek termination; but it may share this censure with many another word which has become an integral part of the language, and for a needful and helpful idea surely a poor word is better than no word at all. I will, therefore, not hesitate to employ it, and will briefly consider the characteristics of the scientist, and his position in the community.

It would be inappropriate here to undertake any philosophic discussion of the position which the scientist should occupy in an ideal or a well-ordered society, or of the duties imposed on him by his assumption of the priesthood, as an interpreter and expounder of the Divine word written upon the tablets of the material universe. Such course of inquiry would imply, as its basis, a determination of the reciprocal duties of all members of society, whatever their calling; and this involves, in its turn, the deepest questions of political economy and social philosophy. We must take certain principles for granted, and among them, this: — that civilized society is an organic body, of which each member is, in spite of himself, dependent upon the rest, and exerts a corresponding influence in return.* The many-sided culture of a normal community is the resultant of the varied capacities, culture and efforts of numerous individuals, no one of whom could attain the highest grade of usefulness in several diverse departments. It depends upon the relative number and variety of the ablest and most cultivated members, conjointly with the influence they exert. Human science and art have, in the progress of our race, advanced far beyond the comprehension of any individual. An equal culture in many directions is synonymous with superficiality in all, and an "admirable Crichton" is today simply a ridiculous object. We cannot well escape the conclusion that — so far as is compatible with that general education of physical and mental faculties and those general attainments which the welfare of others, as well as the amenities of society, require, and which are needful alike for the mental and moral health of the individual — the energies of each one should be consecrated to the development and employment of particular capacities. No thoughtful man can arrive at years of discretion without becoming aware of the character and direction of his mental powers, even though he may be incompetent properly to estimate their relative magnitude; and there can be no reasonable doubt that taste and predilection would afford safe guides for the individual in entering upon his career, were the organism of society fairly developed without distortion by untoward agencies.

Therefore, if the investigation of scientific questions and the discovery of scientific laws is needful or desirable for our race, it becomes the duty of every civilized community to encourage and protect the vocation of the scientist, and it is the duty of those who feel themselves called to this vocation to devote themselves to it with heart and hand. Their commission is from on high. "Freely ye have received, freely give. Provide neither gold nor silver nor brass in your purses, nor scrip for your journey, neither two coats; for the workman is worthy of his meat." If the Creator designs that the race formed in his own image shall discern and comprehend the laws through which he has exerted his creative power; if he means that his wondrous works shall be read by man, upon whom he has bestowed the means and the impulse to read them; if he chooses that our higher capacities shall be cultivated in this world as well as our lower ones; then does he also intend that a class of men shall exist and be maintained, laboring in behalf of all, and devoting their highest energies to intellectual conquests for the race through the study of his works and the interpretation of his laws. Is there any argument, of all those with which the world has been familiar for more than three thousand years in behalf of the sacred ministry of religion, which is not applicable to the ministry of science? If the highest act of the human spirit be to attain to an intimate relation and communion with the Father of spirits, who shall dare discredit that other exalted duty of searching out God through his works, and learning him as he has seen fit to manifest himself to us directly. Unreasonable as it would be to maintain that the word of God, when filtered through many a human tradition and recollection, and translated from language to language after being recorded upon the manuscript, is more surely and emphatically his unperverted word than is that fresh from his own fingers, "written all over the earth, written all over the sky;" how much more so would it be to maintain, that the former, but not the latter, needs a body of investigators. Far be it from me to imply, however indirectly, that the reverse is true — that the culture of the intellect should take precedence of that of the religious faculties — that the most elevated regions of converse with the Deity as known through his noblest works, or his profoundest physical laws, could supersede the necessity of communing through the affections and emotions, or the need of relief to the famishing cry of the soul for bread from its Father in Heaven. It might, perhaps, be urged that for the former a priesthood is, and for the latter it is not, indispensable; that for the former the progress of interpretation goes on continuously, while for the latter little remains to be done, other than the exposition and enforcement of what has already been attained; that the former must lead, while the latter is not unlikely to follow, the development of society. But there is no ground for comparison disadvantageous to either class. My argument is that they should stand alike. Both classes are needed to satisfy a deep and insatiable demand; both are imbued with the instinct to provide a supply. Nor are the abnegations and self-sacrifices by which the one has earned the martyr's crown in past days and lasting reverence in the present, without parallel and counterpart in the other.

With what a spirit of grateful recognition of the Almighty's revelation to the scientist in these latter days, may we read the answer to Job out of the whirlwind!

"Gird up thy loins now like a man, for I will demand of thee, and answer thou me. Where wast thou when I laid the foundations of the earth? Declare, if thou hast understanding. Who hath laid the measures thereof, if thou knowest; or who hath stretched the line upon it? Whereupon are the foundations thereof fastened, or who laid the corner-stone thereof, when the morning stars sang together, and all the sons of God shouted for joy? . . . . . Hast thou entered into the springs of the sea, or hast thou walked in search of the depths? Hast thou perceived the breadth of the earth? declare, if thou knowest it all. Where is the way where light dwelleth, and as for darkness, where is the place thereof? . . . . . Hast thou entered into the treasures of the snow, or hast thou seen the treasures of the hail? Knowest thou the ordinances of heaven? Canst thou set the dominion thereof in the earth? Canst thou lift up thy voice unto the clouds that abundance of waters may cover thee? Canst thou send the lightnings, that they may go, and say unto thee, Here we are?"

Job answered and said:—

"Behold, I am vile, what shall I answer thee? I will lay mine hand upon my mouth. Once have I spoken, but I will not answer; yea, twice, but I will proceed no farther."

But we might reply:—'Lord, thou hast revealed unto us all these things. Us also hast thou taken into the counsels of thy creation, for thou hast not deemed thy children unworthy of thy knowledge. The foundations and breadth of the earth and the ordinances of the heavens, the depths of the sea and the way of the light, the treasures of the snow and the sources of the hail, the sending of lightnings to say 'Here we are,' and the lifting of our summons to the clouds that we may have rain;—behold thou hast disclosed them all unto us, thy children!'

The claim of the scientific profession to recognition and support has long been acknowledged by most European nations. Throughout the European continent organizations have been established, and are maintained by the government at very considerable expense, for the one purpose of promoting scientific research; and the individuals composing these organizations are provided with the means of support while laboring to this end. In our own utilitarian country few claims are recognized excepting such as afford a direct personal benefit, which the individual recipients can estimate by some pecuniary standard. Thus the successful investigator in any special department of medical science may reap a rich reward,—so rich indeed as to hold out strong temptations for the surrender of so large a share of his time and energies to the practice of his art, as to leave small opportunity for the farther prosecution of his science. Indeed, when we examine the matter carefully, we shall find that it is only art (*i. e.* the application of principles and laws), for which any practical recognition can be expected in America at present; while science (*i. e.* the discovery and investigation of these laws), even where nominally fostered, receives support only through some of its indirect branches, which more properly belong within the domain of art. In this way medical science in this country is supported only through individual need of the healing art; physical research, only through its most direct application to technology; mathematical investigation, only in so far as it stands in palpable relation to engineering, surveying, or some other practical use; chemistry, as being an important handmaid to manufacture and to metallurgy; astronomy, almost solely as an assistance to navigation. To one considering these unquestionable facts, the parallel case at Niagara presents itself unbidden:—

> "The tailor made a single note:
> 'Gods! what a place to sponge a coat!'"

In recounting these facts, it is without apprehension that they will be scouted, by any educated and thoughtful man not versed in scientific matters, as though they presented the one-sided view entertained by a narrow class of persons, who, from habitual occupation with abstract or general inquiries, have become blinded to the great material interests of society. No doubt the advance of civilization is measurable by the progress of the arts; and especially is our nation charged, as no nation ever was before, with the duty of subjugating nature, and diffusing the arts of civilized life over a continent. No doubt the first instinct of humanity is to provide for its material well being; and the craving for comfort and luxury is a stimulus at whose bidding the whole world is to be made one family, through the beneficent agencies of commerce. But I do claim, first, that we have reached a stage at which it behooves us to acknowledge a higher aim, as much beyond the commercial and technological as the intellect is beyond the body: that the aim is dictated to us by the Creator through intellectual incentives and opportunities, and that its pursuit is unfailingly rewarded by material recompenses. And, secondly, even if we disregard these considerations altogether, and take into account merely that material progress to which America is devoting all her energies, that it is a narrow and baneful policy which forgets that immediate, direct and palpable influences are not the only ones. Rarely are they even the chief ones.

It would be throwing words away were I to undertake to prove, what you all know already, that scarcely one of all the great advances in the material welfare of humanity would have been made but for the scientist in his closet, whose experiments, researches and generalizations, incited by the love of nature and the aspiration to fathom her laws, have afforded the knowledge which the inventor's fertility of device has made subservient to human welfare. There is no need of balancing the respective merits of the discoverer and the inventor. All will agree that but for the former the latter would be of little use. To be sure it would be false reasoning to maintain that, because valuable inventions are usually due to scientific discoveries, they must be deemed a necessary consequence thereof. Yet experience points to some such conviction; and it would be difficult to point out an important scientific discovery, no matter how abstruse if twenty years old, which has not already conferred some material benefit upon humanity, and which was not itself dependent upon each one of several independent and seemingly isolated previous researches.

It is not then alike the wisest policy and the evident duty of a people already much advanced in material well-being, and ambitious of progress, that it should recognize the debt it owes to science, by courting such opportunities for the future as science may afford? Is it not among its most palpable duties to develop and encourage scientific tastes and investigations, appreciating the material sacrifices which these entail, even under the most favorable circumstances? Is it not discreditable to the civilization of a great people, when scientific ability is habitually stifled or lost, by want of opportunity for its development? Yet a tale might be told, year after year, of earnest and gifted young men compelled by want of bread to abandon the scientific path upon which they had entered with fervor. And if told, I believe it would astonish those whom circumstances have not inured to the facts, as it would mortify and sadden the patriot and the philanthropist. It is to be expected that the pursuit of wealth, or place or power, should have its own reward; the sincere follower of science neither seeks nor expects any of these things. But, unless the idea that a community can have duties and responsibilities, as well as an individual, be preposterous, the competent and willing votary of science is entitled to the means of investigating while he lives, and of living while he investigates. In all this broad land I know of not half a dozen positions, the duties of which may be discharged and a subsistence earned by prosecuting scientific research. What is done is in the intervals of leisure from other labors, which exhaust the energies, but upon which the investigators are dependent for support. The collegiate professor, whose nervous vigor is expended in the task of instilling trite rudiments into the minds of enforced pupils, forms no exception to this statement. And where are the opportunities for those higher teachers who would fain use the blessed privilege of training others for the scientific vocation, and be rewarded by the consciousness that their conceptions, methods, plans and perhaps conjectures, are not to die with them.

It would be far from reasonable to expect that the labors of any considerable proportion of investigators should be severally crowned by large generalizations, or by such discoveries as are subjects of popular appreciation. These are comparatively rare, and have always required the antecedent researches of a series of investigators to whom history is wont to award inadequate acknowledgements. Stones must be quarried and hewn before the edifice can be reared; facts must be gathered before their laws can be discovered; the several laws must be recognized before their generalization can be effected. The function of the scientist is to attain new truths by conquering them from the limitless realm of the unknown, and whether they be brilliant or otherwise, none is too small or insignificant in aspect to deserve his earnest search or joyful welcome. Among the marked characteristics of modern science are its recognition of the value of every observation and experiment,—since there is no one which may not afford a basis or a clew for subsequent advance,—and its appreciation of the services rendered by those who lay the stepping-stones requisite for continued ascent. And in proportion to the scientific development of a community is its relative estimate of the men who do not disdain those minor researches which are yearly becoming more indispensable, yet are unrewarded by popular applause. The science of the nineteenth century is to be sought not in ponderous tomes, but in abundant memoirs. Scientific progress in these days is like that of a besieging army. Little by little miners work beneath the surface; slowly the intrenchments grow to right and left, approaching always, however indirectly; gradually the long circumvallations close around the citadel. Here, ground is gained for a new base of operations; there, is opportunity for striking in a new direction. Through avenues thus laboriously prepared the embattled host advances. At last a point is secured whence the artillery may begin its work. Under cover of this, new approaches are effected, until at last, in the fullness of time, the final charge is made. One brilliant dash, and the stronghold falls. But should the engineers' devices, the miners' toil, the soldiers' labor at the earthworks and the artillerists' service at the guns, all be held cheap, because of our admiration of the gallantry and chivalry that led the decisive onset? So thoroughly has the scientific world been impressed in recent years with the importance of judging researches not by their brilliancy, but by their promise of ultimate usefulness, that the straining after showy results is deemed unworthy and derogatory, while a new and well established fact is welcomed with a more earnest cordiality than the most fascinating hypothesis, or the most plausible conjecture, prior to its verification.

All science is, and must be, to a certain extent, experimental. Even in those branches which from their very nature preclude arbitrary experiment, comparisons of phenomenon with prediction, of observation with computation, supply its place. Methods of discovery, in astronomy and in chemistry or terrestrial physics, differ but little. Keppler's mind was scarcely of a different order, or his processes of a different class, from Faraday's. And, essential as the inductive method may be for a control and criterion of the results attained, few discoveries were ever made by pure induction. Given the result in advance, experiments may be contrived for guiding to it; let us once know that the truth lies in one, and one only, of two divergent directions, and we may think out crucial instances. But such are not usually the circumstances under which discoveries are made; and learning and skill are in general no more necessary for disposing of hypotheses, than is ingenuity of invention for framing them.

The facts and relations to which I have called your attention indicate the functions of the scientist in the present condition of our civilization. The duty of investigating the principles and laws of the material universe once conceded, this office devolves, in the grand division of labor, upon a special class. And that stage of progress is already attained, in which subdivision in a high degree, among this class, is imperative. Large acquaintance with kindred branches of science, and special concentration of effort upon a narrow field, are alike requisite for the investigator. Moreover he must be content with small and modest additions to human knowledge; humbly and hopefully gathering what he may, and bringing faithfully his sand-grains to the heap, if he find no stone for the temple. He may no longer look for brilliant discoveries as the sure reward of earnest research, though he should possess the genius of Pythagoras, Archimedes, or Copernicus; nor have others the right to expect it of him. And if perchance any such discovery or generalization fall to his share, simple justice demands that he concede to others much of the merit. The activity and energy of scientific inquiry at the present moment are intense beyond all precedent in history. The accretions and developments of a couple of years change the aspect and relations of each successive discovery almost past recognition. An important fact, noted but unpublished by one man, speedily manifests itself to others; so that suppressed discoveries are in fact abandoned ones, and the most important are very frequently made in duplicate. This is simply because the limit of our human knowledge spreads like a great circular wave, emanating from a center. The advancing lines have access only to what lies upon the margin before them, and the throng who press forward tread in contiguous paths, the divergence of whose radiation is overbalanced by the continually increasing number in the ranks. And it is characteristic of the present time that it is a period requiring co-operation and associated effort in scientific research, not merely for the sake of needful distribution of labor, but because combinations of resources and acquirements are requisite, to which no individual can attain.

It would be a grateful office to congratulate you upon the part our own people is taking in this great campaign for intellectual conquest. Thank God we may claim some part in it, and names too, among the living as well as the dead, which will surely gain lustre in scientific annals with the lapse of time. Yet how small is our relative share! Why have we not, in our forty millions of men, as many active investigators, as many scientific institutions, as much national support, as much popular sympathy, as may be found in Germany, France, or England? Why are the efforts of the scientist appreciated and encouraged solely in proportion to the estimate of them by popular and altogether incompetent tribunals? That it is so, needs no demonstration. Popular sympathy or encouragement rarely rewards the scientific investigator while living; and when it does, it is seldom because of his highest achievements. And those rewards, which the community honestly desires to bestow upon this class of intellectual labor, are but too apt to fall to the mere bookwright, if not to the charlatan. Meanwhile great public interests suffer for want of such guidance as very many easily might, and gladly would, give who live and die unrecognized by those who would desire to make their services available, and whose recognition would augment their usefulness a hundred fold.

Do you ask the remedy for this disease? I know none, except such change in public sentiment as may lead to deference, in scientific matters, to the judgement of experts, together with the maintenance and encouragement of institutions which may serve to develop experts and indicate who they are.

It cannot be denied that there is a large class with whom strong antipathies exist against scientific pursuits, and against those who are habitually engaged in them. For these prejudices there are various reasons, some by no means unnatural. That continual demand for evidence, which scientific studies evoke, is peculiarly distasteful to the vague and purely speculative mind, and affords a never-failing subject for ridicule. The very warfare in which the reverent votary of science sometimes finds himself involved in defence of her interests — so paramount in his estimation to his own — is made a ground for imputations of irritability and jealousy. The mere utilitarian objects to the abstractions of the higher sciences, as unfitting men for the daily duties of life; ignorant himself of any utility or duty which does not lie upon the surface. And then there is a counter-prejudice even among some educated and thoughtful men, which regards all studies pertaining to the physical universe, as of a low order and "materialistic" tendency, and which actually despises all inquiries, the correctness of whose results may be tested.

How far just foundation may exist for any of these adverse judgements, I will not undertake to say. There can be no doubt that exclusive attention to any one class of pursuits, will give onesidedness to the character, as well as to the culture; yet I am not sure that scientists are more justly subject to this criticism than any other class of men. Indeed it might be claimed for them, with a fair show of reason, that as a class, they are more familiar with literature, philosophy, and the arts, than the followers of these vocations are with science. Certainly they are better acquainted with the practical affairs of technology, than the so-called "practical man" is with scientific matters. And if the tenor of their studies lead them to distinguish sharply between what is, and what is not, susceptible of proof, and thus to lose some of the genial influences of imagination through their quest of the ennobling influences of truth, they may perhaps be thereby rendered less agreeable companions, yet they are none the worse citizens.

But all the opposition to scientific studies, springing from such considerations as I have named, is small compared with that from theological sources. From the very dawning of modern science, the various systems of theology have waged against it an unceasing war. The demonstrable character of its results have rendered it especially obnoxious to those who feared some encroachments upon their prescribed tenets, or who dreaded lest the overthrow of some favorite theory might be fatal to all reliance upon their creed. Some have assumed an antagonism between the orally and the visibly revealed word of God, and in their solicitude for the former have attempted to discredit the latter, and summoned to their aid the whole battery of the casuist. But they have forgotten that excess of zeal is abortive, whether it be in behalf of science or of religion, and is sure to create a reaction injurious to that cause in behalf of which it is exhibited.

I remember thirty years ago hearing a venerable and good man, who stood in the front rank of our scientific teachers, preface his lectures on geology with a deprecation of its "infidel" tendency when not properly interpreted. And the days are by no means past, when the efforts to reconcile apparently conflicting statements, in the book of Genesis and the book of Nature, are far more strenuous than any efforts at verifying the credentials of either statement. The inquisition of two decades ago took a different shape from that of two centuries earlier, but it was scarcely less tyrannous or unrelenting. That agony of nerve and muscle, which wrung from Galileo Galilei his transient recantation, was scarcely more severe than the mental and moral pangs, which more than one man of science has been called upon to bear in our own day, because he has become convinced that our earth existed thousands of centuries ago; that all mankind are not descended from a single pair; that the evidence is decisive that human beings lived during the pleiocene period; or that the sun antedated the earth, and its alternations of day and night.

All this is changed in our seminaries of learning today, I admit. In the meridian blaze of scientific knowledge, and the diffusion of its life-giving energy, such darkness could not continue. Yet how short the interval since its dispersion! Revulsions of popular feeling are like the oscillations of a mighty pendulum. Reaction has now in turn overshot its mark, and today it is theology which stands on the defensive. Even that attempted compromise, which would leave to scientists all things scientific, and to theologians all things theological, although in its nature unstable, has failed to find that temporary acceptance which might have been anticipated for it.

Thus the conflict, between accepted creeds and facts claiming to be demonstrable, is one which may no longer be delayed. No temporizing will avert it more, and one or the other must yield. Truth, though many-sided, cannot be discordant, and the honest man desires to know and to accept it. No evidence for any theory can be satisfactory, so long as evidence on the other side cannot be disposed of. The student of nature deals, it is true, only with material facts; yet his results, such as they are, are demonstrable, and may not be discarded to suit the preference of any sectarian. On the other hand, theological and philosophical inquiry deals only with moral evidence and with mind; yielding results which rarely admit of actual demonstration. With a strange avidity, the name 'science' is constantly claimed for researches in these fields, as though no other word were equally honorable; still it is relatively only a very small portion of them to which the term may be rightfully applied, since it implies the investigation, not of facts alone, nor doctrines, but of laws. And although unquestionably there have been, and may be, elicited in this field certain laws, yet their paucity is recognizable by the criterion that what is once established must be accepted by competent minds, and adopted as a basis for farther research. To deny the laws of gravitation, of the tides, of storms, of magnetism, is merely to manifest ignorance. Yet the multitude of diverse systems of philosophy and of religious creeds has been increasing, not diminishing, for the last two thousand years.

Still it may not be denied that we have two independent means of attaining knowledge of the higher truths. These imply totally different methods, and should reciprocally confirm each other, if their results be correct. Let the existence of a real conflict in their results be once established, and their is no escape from the inference that at least one of them is erroneous. Now although mistakes in scientific deductions are frequent enough, no sane man will contend that those results are erroneous, which may be proved to the acceptance of all competent investigators who examine the question. And those, who war upon all scientific investigation of theological questions, must either object to competent evidence, or must plant themselves upon the dangerous ground that all physical evidence is inadmissible.

In what I am saying I am sure no one will suspect me of the slightest intentional disrespect to the religious convictions of any earnest believer, however conservative or however liberal. Yet we are continually brought to the old, old dilemma, where science seems to demand one inference, and faith another. To accept either, if hopelessly contradicted, is repugnant to the philosophic mind; how to reconcile them, has been the problem of the ages. The apparent antithesis may be variously stated. One man presents it as between Nature and Revelation; the latter certainly divine, the former in its essence illusory. Another puts it as between Science and Religion; the former ignoring the moral, and the latter the perceptive faculties. A third gives it as between the evidence of the Senses and the intuitions of the Soul. Bigots, casuists, fanatics, have each in turn assailed the teachings of science, and have swept along with them many a good and earnest man whose fervid piety has led him to glory in the motto '*Credo quia impossibile est.*' Vague arguments, in which words and ideas have become almost inextricably confounded, have alarmed the consciences of men, lest the rewards of faith should be withheld from those whose faith required no sacrifice of reason. Meanwhile it has been forgotten, both that there must be some apparent reason for one's faith, and that faith and reason are alike expressions of the Divine in man. To such an intensity has bigotry continued even in our own days, that we may see the mischievous sophistry yet maintained in some quarters, that there are moral limits to inquiry, which man may not presume to transcend; before which scientific research is unlawful, and theological inquiry sinful. What could impel to such doctrine, except an apprehension of those results to which honest research might lead? Were the prohibition addressed only to a class deemed incompetent to investigate intelligently, or whose preliminary knowledge was inadequate, we might appreciate and possibly approve it. But this is not its spirit. It is either an assertion that the Almighty cannot guard his own secrets, or else it is an assertion that the All-wise and All-powerful has imbued us with quenchless aspirations, and has established incentives which cannot be followed without leading us away from him. Before modern science existed, while superstition held unchallenged sway, when dealers in the black arts of chemistry, astronomy, and the like, were conceded to have sold their souls to Satan, such a doctrine might not have surprised us. But that educated partisans of any creed should maintain today, that obedience to the God-given instinct of searching out the laws and being of the Almighty, in his physical and moral creation, is a crime, and that the tree of knowledge still bears fruit which it is forbidden to gather, in however reverential a spirit, seems a horrible anachronism.

If there be one moral truth which may be regarded as beyond all question, it is that our worship belongs to the author of nature, who fashioned alike the body and the soul of man, and is sovereign over all matter as well as all spirit. Yet the separation between scientific and religious views of the universe has been growing wider for a century, impelled by the joint efforts of the bigot and the atheist, who have worked most earnestly together for their common object. Never was the need so sorely felt for the discovery and recognition of that middle term, which must exist, and through which the dissonant views are reconcilable.

One of the clearest thinkers of our time believes that he has found such a middle term, and maintains with vigorous argument the doctrine that it exists in Force. And among its varied and correlated manifestations he claims that it nowhere finds a simpler or higher manifestation than in Will,—the only form which may claim to be primary. Here we enter upon a theme so lofty and so dizzying in its height that we may well distrust our powers; and in the unwonted keenness of our sense of their inadequacy, we feel how closely we are treading upon the margin of their range. Yet I will venture to say a few words upon this point, because I find myself alike unable to resist the conviction that a great verity underlies the idea, or to accept the doctrine as consonant with what may be regarded as demonstrated concerning the nature of force; notwithstanding the ability with which it has been maintained by a certain school of philosophers, that life, consciousness and all psychical energy are simply manifestations of this same force,—convertible into and deduced from heat, chemical action and the like.

Scientists are now of accord that "force can neither be created nor destroyed," and that "the quantity of force in nature is just as eternal and unalterable as the quantity of matter." Its various forms are eminently convertible, yet utterly indestructible. And to avoid that fruitful source of disagreement among the ablest men, which has arisen from the ambiguous signification of the word, we must adopt the meaning which is finding general acceptance, and define force as "that which is expended in producing or resisting motion;" thus clearly discriminating between force and its cause.

In his retiring Address before this Association, last year, our honored ex-president Dr. Barnard presented an argument, so vigorous and clear that I see no room for an adequate rejoinder, in opposition to the doctrine which would extend the principle of the conservation of force to the phenomena of consciousness,—"a philosophy which at the present day is boldly taught in public schools of science, and which numbers among its disciples many very able men." He says, for instance:—

"Organic changes are physical effects, and may be received without hesitation as the representative equivalents of physical forces expended. But sensation, will, emotion, passion, thought, are in no conceivable sense physical."—[*Proc. Amer. Assoc., Chicago, p.* 89.]

"The philosophy, which makes thought a form of force, makes thought a mode of motion; converts the thinking being into a mechanical automaton, whose sensations, emotions, intellections, are mere vibrations produced in its material substance by the play of physical forces, and whose conscious existence must forever cease when the exhausted organism shall at length fail to respond to these external impulses."—[*Ibid, p.* 91.]

"Thought cannot be a physical force, because it admits of no measure. . . . A thing unsusceptible of measure cannot be a quantity, and a thing that is not even a quantity cannot be a force."—[*Ibid, pp.* 93, 4.]

Before the cogent reasoning carried out by President Barnard, of which the general tenor is indicated by these quotations, the view that force affords a middle term between the moral and the material worlds can be sustained as little as the pure materialism against which the argument was directed. But if we ascend a grade higher, and consider that which guides and compels force, as force guides matter, I am disposed to believe that the problem may be nearer to a solution. Yet I offer my views with hesitance, not unmindful of the great thinkers who have considered these exalted topics, and shrinking from the rebuke of presumption.

There is an elegant experiment, in which the tension of a spring is made to produce heat by percussion, thus developing the current from a thermo-electric battery, which by successive modifications of its force exhibits heat, chemical action, magnetic attraction, and finally bends another spring; the same original force successively appearing in all these various manifestations until it is reëstablished in its primitive form. In such an experiment the imperfections of the apparatus would of course entail some loss at each successive step, and thus preclude the practical recovery of an available force equal to that expended in the original flexure of the spring. Yet the fact is beyond question that such loss is due solely to the inadequacy of our implements for collecting and transmitting the force at each stage of the experiment; for the law of conservation teaches that it is in every instance converted into other form or forms without diminution. Could such an apparatus be constructed with theoretical perfection, it would represent an eternal circuit of force; and, like the frictionless pendulum in a vacuum, it would exhibit a perpetual motion, after the needful impulse had once been applied. The spring would oscillate forever, did no extraneous force oppose, whether the force producing its rebound were or were not transmitted through a chain of modifications.

In this inert apparatus no force whatever would have been embodied, yet qualities would have been implanted by design, which would compel an indestructible force, applied to it, to play the part of an unwilling Proteus. The influence seems unavoidable that force may be guided and controlled, compelled to exert itself in this or in that shape, without the outlay of any other force for the purpose. If it be objected that it is an intrinsic law of force that it shall change its form in exerting itself, the case is in nowise altered by the expression of this truism. Our design has prescribed, and (extraneous force being absent) might indefinitely prescribe, the modes and directions in which that constant force should manifest itself.

Muscular force is directed, and in its vital action is usually controlled, by will. If we assume it to be coequal with the expenditure of tissue,* measurable alike by its transferred results and by the decomposition of this tissue, where and what is that power which lets loose or withholds this force, and whose action is attended by a conscious effort? It is the will, —a something which directs and controls force without expending it. Not only are thought and forms of consciousness not forces, if the reasoning already adduced be correct, but, although often moral incentives to the will, they are not even motive energies, in the sense in which I think we must concede the will to be such. It is true that the exercise of thought is followed by fatigue, yet it is not attended by a sense of effort, except in so far as it is directed by an exertion of the will. And although the former doubtless consumes tissue, have we any reason for believing that the exercise of will does the same, apart from that consumption which corresponds to the forces whose mode of action it prescribes?

Thus it would appear that the metamorphosis of force, though not "work done" in the mechanical sense, is the result of some definite mode of causation. What this causation is, and whether it is susceptible of measurement, are the next questions. In the same category with this agency, or energy,

* Even if it be also, to some extent, supplied by the disorganization of food not fully converted, the argument is not thereby affected.

or influence, the vital principle would seem to belong,—directing forces while it neither expends nor consumes them. In the growth of organic beings, unstable combinations are formed, and organized structures are thence reared, in which, as Kant has so beautifully said, "all parts are mutually ends and means." If in such organic development force is consumed, disorganization without decomposition ought to evolve it. Of the deposit of force in the unstable material of the tissues, I am not speaking, but of the vitality itself, which represents an energy requisite for the development and growth of organisms,—their dissolution being in turn attended by development of inferior forms of life, which suggest that this energy may have again been made available,—an energy too which is not "force," as this term has just now been defined.

No comparison can be drawn between vitality and those molecular forces which build the crystal. Crystalline forms arise when the molecular attractions enjoy the freest scope, and their construction must be attended by an evolution of force, which ought to be recognizable by physical tests, and which should also be measurable by an excess of their resistance to solution, over that of comparatively amorphous masses of the same material, in which equal weights present equal surfaces.

So, too, not only in that individuality which life confers and in the impossibility of insulating or transferring vitality, but also in its hereditary character and its apparent susceptibility of indefinite increase or diminution, the vital energy violates our fundamental conceptions of force, and demands a separate category, seeming to belong in the same with will. If will and life be forms of force, their total amount must be limited by the law of conservation. If, on the other hand, they are outside the realm of forces, we may more readily indulge the conviction to which experience would lead, that their freedom is unfettered by any restrictions within our knowledge,—each enjoying an indefinite, though possibly a correlated scope in its own domain. The indestructibility of both matter and force implies a fixed coefficient of force for matter in equilibrium; but how great is the contrast offered in this respect by such energies as life and will!

Now if this reasoning be correct, we may have in this class of energies that middle term, so earnestly desired and so intensely needful, which unites the phenomena of matter with those of spirit, and forms the connecting link between science and religion; their harmonious conjunction affording the highest system of philosophy. It is this class of energies which, controlling the forces of matter, guides and governs their modifications and transformations. It is this, moreover, which, inseparable from mind, is exerted by all conscious organism. The mystic play of coequal, but to our senses, so dissimilar forces, and the equally recondite mutual action of the eye, the brain and the nerve, alike demand agencies transcending all our science, yet implicitly obeying physical laws. The highest manifestations of these agencies is in will; the highest agent is the Almighty. Thus the dictum of faith, that the universe exists only by virtue of the continued will of its Creator, represents a palpable scientific fact; and we may see that the pantheist, the materialist and the spiritualist (I will not be debarred from this noble word by the associations of its misuse today) have been contemplating the same exalted truth from different aspects, with limited ranges of vision.

With the disappearance of theological hostility to science, a new era will commence, and increased progress may confidently be expected both for science and for religion. But we may not conceal from ourselves that the omens are less favorable for science in our own land than elsewhere, since there are peculiar obstacles to be encountered. These chiefly arise, directly or indirectly, from that characteristic in our national development, which assigns an exaggerated value to immediate utility, and a low estimate to what real utility is. It cannot be denied that the attainment of riches is becoming with us more and more the chief aim of existence; and this tendency is aggravated by that dominant spirit in our large cities, which gives to wealth alone the influence which it ought to share with integrity, with refinement, with education, and with talent. Thus the ambition of our youth is almost irresistibly directed to the acquisition of property as the highest worldly good, and their experience is made to confirm the doctrine. Our institutions of science, few as they are (and almost universally confounded by the public with institutions for the preliminary education of youth), are, like the latter, dependent upon subsidies and gifts from individuals. A disposition to advance the public welfare by liberal munificence, may certainly be claimed for our wealthy classes, and America has a right to abundant pride in the generosity of her rich men; yet it cannot be expected that largeness of views in literary or scientific matters should always be commensurate with largeness of heart; and the monuments of unwisely directed generosity, scattered all over the land, commemorate gifts, which, if judiciously bestowed, would probably have placed the United States in the front rank of intellectual progress. But, more than this, these same influences have resulted in placing, to a very large extent, the governance and guidance of intellectual agencies, and the control of intellectual institutions, in the hands of men not well fitted for their exercise. How could science, or literature, or art thrive, while their interests are in the keeping of those who do not comprehend them, and who, even with the best intentions, do not know at what to aim? The administration of the finances of an institution loses much of its value, when the institution itself goes adrift. And then again, while it has been and is still the usage to do all we can for the education of youth up to a certain point, no encouragement or support has been deemed needful beyond that point; so that in fact the individual is aided in preparations for usefulness, but receives not the slighest encouragement for the actual exercise of that usefulness, after the preparation is completed. In late years we have actually retrograded in this respect; and, even in seminaries of education, discouragements are thrown in the way of those studies which do not suit the views of the utilitarian. But in America today, the crying need is of opportunities to make serviceable such preliminary training as may have been acquired in any purely intellectual or scholastic field — a need far greater than any lack of opportunity to obtain this requisite education. It has become forgotten that the training of the school and the college is but a means, and not an end; and, as in so many other cases, the end is lost from sight in the pursuit of the means, — the prosecution of research is neglected, while the requisite education is zealously provided for. Thus it is that the scientist is compelled, almost without exception, to earn his bread independently of his vocation, that is to say, by work other than scientific research. Finally, the want of any recognized tribunal, whose judgement might be provisionally accepted upon matters requiring scientific knowledge for their decision, which might command public confidence by the character and attainments of its members, and which could represent, advocate and maintain the interests of science with the public and with government, has till recently been a source of disadvantage, which it would be difficult to overestimate.

The extent to which our people has indirectly suffered from the want of recognition and support of scientific pursuits would scarcely be credited at first statement. The lack of those mental habitudes which they imply, is a fruitful parent of superficiality, and is largely accountable for our national fault,—want of thoroughness; for thoroughness will never flourish while only those pursuits are encouraged which promise immediate recompense of the most tangible sort. Another result is the absence of deference to competent authority, the absence of respect for mental excellence and power. To become aware how little we know, requires some progress in knowledge; and, just as want of faith brings superstition, so disregard or ignorance of known laws begets credulity. What ludicrous illustrations of this fact are daily offered by the vague popular conceptions of the well-known laws of electrical action. I have heard it said that the most complex processes are suddenly revealed and made clear to the feminine mind by the magic word "machinery." Be this true or not, it will scarcely be denied that to the masculine intellect there is a cabalistic virtue of equal potency in the mysterious name "electricity." To this awe-inspiring agency — although no more recondite for the physicist than heat or light or gravitation — all uncomprehended facts are attributed with a sort of satisfaction to that reference to supernatural powers is thus evaded!

The recently prevalent belief in ghostly table-turnings, supernatural knockings, rappings and bell-ringings, and in spiritual lead-pencils, would furnish also a painful illustration of my meaning, were it not still a somewhat dangerous subject to discuss with freedom,—even out of Salem!

Prominent among the serious dangers which are theatening the welfare of science among us, at present, is its advocacy upon improper grounds. That man is no loyal follower and no true friend, of science who bases her claims to support upon the ground of immediate practical utility. How essential she always has been to the useful arts, all history and experience proclaim; yet these recompenses would form indeed a low and unworthy motive for her pursuit. To follow her for such an end would be to follow a Divine leader for the sake of the loaves and fishes, expected to be miraculously dispensed to hearers of the word. To hear that word, to learn that law, to gain some comprehension of the lofty scheme, such are at least the only motives worthy of avowal. The present reaction from the era, when all culture other than classical and metaphysical was, disregarded if not despised, and the crusade against classical culture, which results from this excessive reaction, bode no good to science. The champions in this crusade occupy simply the utilitarian ground, and their alleged advocacy of science is in fact scarcely more than an advocacy of the useful arts as the highest object of education or of the attention of the educated classes. The crusade is not in behalf of this or that form of intellectual progress; it is against such intellectual culture as has not some tangible end, capable of being represented by dollars, or finding expression in some form of physical well-being. Results of this outburst of utilitarianism, combined with worship of Mammon, are already visibly manifested all around us in the substitution of expensiveness for elegance, of monstrosity for grandeur, of gaudiness for beauty, of quantity for quality. As the golden age degenerated to the iron, so the age of iron has dwindled into that of tinsel. See, in so many public buildings, how tawdry contrasts in color, or extravagance in ornamentation, usurp the place of beauty in form. See, in public grounds, how the grace and harmonies of nature are ostracized, for the sake of putting something expensive in their place. Even the reverence which would fain preserve and protect what is hallowed by associations and memories of the great and good is regarded as a conservatism, quite "behind the times."

To save our country from the abyss, on the verge of which it stands, will require all the energies which can be summoned; yet we have the satisfaction of knowing that, with few exceptions, the most refined and cultivated men of the land recognize the danger and are united in efforts for its aversion. Science has few stronger friends than among the scholars of America; scholarship, few more zealous adherents and admirers than among her scientific men. Intellectual culture, of any sort which aims at something higher than narrow utility, is what we need, and its advance in any one direction can scarcely fail to be followed by advances in others. Scientific education, moreover, peculiarly requires fullness of culture; and whatever hampers this, obstructs the progress desired. The experience of ages may not lightly be disregarded, and we must remember that novelty is not necessarily excellence in philosophy, education or art.

But there is a pleasanter side to the prospect; for, where science does have a foothold, her path is becoming smoothed and the sphere of her influence extended as never before. In scientific matters at least, we are attaining the epoch of simplicity, which entails universality, and this in its turn promotes the brotherhood of all who serve a common cause. The magnificent discovery of the correlation of the physical forces weaves the physical sciences into harmonious relationship, and opens to our vision glimpses of still grander generalizations beyond. Recognition of the equivalence of these different forces entails the introduction of absolute units which command universal acceptance; and thermic, electric, magnetic, chemic, mechanic energies are gauged by units depending on the meter and on the earth's rotation. The metric system of weights and measures, already of almost universal use for purpose of research, is rapidly finding popular adoption among all nations, notwithstanding the force of prejudice and the reluctance to modify habitual usage. Thus the nations are entering into more intimate intellectual relations with each other, at the same time that, by the progress of the arts of life, the physical barriers which separated them are broken down, and the sharpness of dividing lines is softened.

It would be unjust too, did we fail to acknowledge the influences by which trade has not unfrequently exerted a most healthful stimulus upon scientific research, when the requirements of the arts have pointed out the directions in which farther knowledge is especially needful for their purposes. The wonderful additions to our knowledge of the laws of electrical circuits, which have been evoked in England by the direct influence of companies for the manufacture and use of submarine telegraph-cables, furnish a brilliant example of what may be accomplished in this way. And of a quite analogous character are those national influences and characteristics which tend to the especial promotion of particular branches of science, and, by the reciprocal and reflected action of these upon the tendency thus implanted, render it markedly prominent. Thus the need of discovering and making manifest the mineral wealth of this continent, together with the magnificent fields offered for exploration, have given in America remarkable development and impulse to geological investigation, and the proportion of geologists among our scientific men is probably manifold larger than in any other country. The same may be said of physical geography and of geographical and topographical exploration. But, more than any other art, war has stimulated physical science; and those branches which have been made to contribute most abundantly to military ends are those which have thriven most among military nations. Applied mathematics and the departments of physics useful to the engineer, the topographer and the artillerist have specially flourished in France. An amusing illustration of the relative positions which sciences and arts may occupy under peculiar influences, is furnished by a publisher's book-list, published in Paris monthly. The newly published books are assorted by subjects, and one of the groups uniformly appears as follows: "*Sciences Mathématiques et Militaires:—Astronomie, Arithmetique, Marine, Equitation;*" thus showing how thoroughly the system of classification is arranged from the standpoint of the cavalry-man. National legislation, too, exerts a decided influence, and in our own land by no means a favorable one to the investigator. At this moment the import-duty imposed by law upon apparatus intended exclusively for investigation, in increasing the sum of human knowledge, is nearly three times as great as that upon the same apparatus if imported for purely educational purposes, in disseminating knowledge already attained!

In these remarks, Gentlemen, I have endeavored to lay before you such facts and considerations as may illustrate the position and relations of the follower of science, especially in the United States. What measure of confidence my inferences may deserve, your own experience and judgement must decide; but I am sure you will not doubt my earnest desire so to present them as to wound the sensibilities of no man. I have sought conscientiously to describe the present aspect of scientific culture in our country, neither shrinking from the statement of unwelcome truths, nor refusing admission to the hopeful promises of the future. What that future is to be, rests in great measure with the generation now upon the stage.

The magnificent, the stupendous march of scientific discovery in the recent past, leads to brilliant and almost limitless aspirations for the near future. The range of human insight into the creation has been of late so wondrously expanded at each limit, that we are emboldened to expectations of scientific discovery, which at first seem utterly extravagant. If within a decade we have learned to analyze the incandescent substance of sun and star, comet and nebula,—if we have attained to thermic, electric and acoustic tests, delicate beyond the apparent reach of human perception,—if we have learned the strange relation of meteor and comet, and added even molecular forces to the lists of known cosmical agencies,—if we have traced the laws of thermal refraction within conducting solids, and found out a higher alchemy in the transmutation of forces,—is it too much to expect that a few years more will disclose the subtle relation between conduction and induction; that whatever may correspond to refraction in electricity will be developed; that the source of the phenomena of terrestrial magnetism will be brought to light by the continued study of its laws; that the mystic bond of gravitation may be made less incomprehensible; that, if radiant action without a medium be possible, its mode of operation will be discovered; and that perhaps even the chemical constitution of the luminiferous ether may be analyzed.

In our own country, none of the obstacles to proper scientific development are insurmountable in their character. Serious they are indeed, yet far from discouraging. Amid our hopefulness and faith in the magnificent future which awaits the land we love more than Jews loved Zion, and toward which we look forward with an intenser pride than Athenian or Roman ever felt in the past glories of his country, we are too apt to tinge all the prospect with roseate coloring; and, in the greatness and depth of our confidence, we are tempted to shut our eyes to inauspicious omens. The patriot's duty is not to deny, but to meet and avert, all danger threatening his country; it is to labor for her welfare, not to bask in dreams of her coming glory.

There are already abundant indications of a desire in the community to encourage science. However large a share may have fallen by the wayside, or on stony ground, still much of the liberality already manifested must bear good fruit. And as the fruit ripens and the community receives the resultant benefit, many of the evils already enumerated must infallibly be diminished. That completely vicious circle of administrative policy must sooner or later disappear, by which, in institutions established for noble ends, the aims and objects themselves are lowered for the sake of winning donations for additional endowment. The time cannot fail to arrive when literary, artistic and scientific matters shall at last receive the guidance of literary, artistic and scientific men. Of all intellectual pursuits, our national character seems most inclined to those of science. Would that the prospects of classical culture and refinement were one-half as good as those of scientific progress; for the proper mutual relations once established, these could not fail to reinforce and supplement each the other. In short, we need only the adaptation of our hitherto untried forms of social organization to intellectual as well as material interests,—an adaptation, which the relatively small influence of intellectual pursuits thus far has too long delayed.

The fundamental idea of this Association is the Advancement of Science by promoting intimate relations among those who love and desire to serve her, by gathering from distant regions the various results of scientific study for common discussion and comparison, and by disseminating throughout the country a popular interest in the ennobling pursuits to which we offer our allegiance. The absurdity of casting ridicule upon the Association because it invites to its ranks all lovers of science, whatever their sphere of life, their attainments, or their avocation,—cordially welcoming the contributions and encouraging the attendance of all who avail themselves of this means of furthering the great end,—is only paralleled by that other one, which finds another occasion for ridicule in the policy of confining the decision of purely scientific questions to those who are scientists by vocation. The flippant sneers at scientific institutions of both classes, which not unfrequently meet our eyes, would almost lead us to doubt whether it were desired to confine all scientific culture to a class of Brahmins, or to submit the law of gravitation to a popular vote by majority. In the present critical period of our national development, the need of an organization like this is palpable; and, if only some element of greater stability and of established policy could be introduced (which would seem by no means difficult), its usefulness would be beyond description, reaching to every corner of the continent, and permeating the whole people with its healthful influence.

Our field, Gentlemen of the American Association, lies clearly mapped out before us. Our duties are shrouded in no uncertainty. To disseminate and impress the great truth that God has given us His works to read and His laws to learn; to advance the public estimation of scientific research, not as a means, but as an end,—an end, however, which when honestly pursued never yet failed to bring rich recompense to the community; to encourage and assist all institutions established for the increase of human knowledge; to inculcate respect for learning and reverence for authority; to guide ambition away from the mere accumulation of lucre and toward intellectual aspirations; to deserve the confidence and guide the liberality of good and patriotic men, who would contribute of their own abundance to further the holy cause of science; to protect scientific interests from the greed of those, who would make of them a prey, incited by the lust of money or of power; and by these and all other righteous means to hasten the time when the land of the setting sun shall become the Orient by leading the science of the world, awing the nations rather by her intellectual achievements than by her material power; — these are among the great interests which are committed to our charge.

May we, one and all, so acquit ourselves of these high responsibilities, that coming years shall render a verdict that the republic has received no detriment through negligence or weakness of ours!

PROCEEDINGS

OF THE

SALEM MEETING, 1869.

COMMUNICATIONS.

A. MATHEMATICS, PHYSICS, AND CHEMISTRY.

I. MATHEMATICS.

1. A DEMONSTRATION OF EUCLID'S ASSUMED AXIOM RELATIVE TO PARALLEL LINES. By ALEXANDER C. TWINING, of New Haven, Conn.

THE plain and admitted fact that science feels no defect in its conclusions and experiences, no limitation even of its most subtle and recondite researches in consequence of the acknowledged absence of a link in the rigorous chain of Euclid's demonstrations, does not, apparently, abate the zealous efforts of modern geometry, to have the defect supplied. It may be hard to explain why the human mind should persist in striving after a desideratum which is not felt as an operative necessity, except we recognize a profound conviction of the superiority of pure intellect, upon abstract subjects of thought, to any and all deductions from mere experience. The author of this paper has ventured before,* and ventures now to follow in the wake of the many who have attempted to prove, under the definitions, and by the severe paths of the old geometry, the *noted axiom*—not so accepted however by modern science, nor yet proven to general satisfaction—that *through a given point there can be but one parallel to a given straight line.* In preparing this paper for publication, some substitutions and abbre-

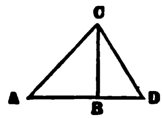
* American Journal of Science and Arts, Vol. i, 2d series, p. 89.

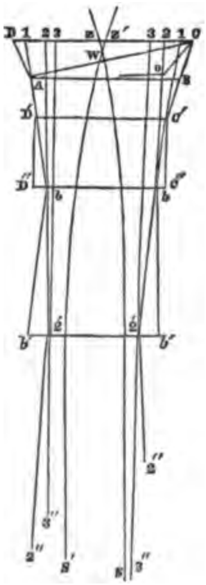
viations have been made, of which an indistinct intimation was given at the time of its reading. Obviously the chief novelty and principal importance are centred in the resulting proposition, which the *Lemma* here immediately following is made to precede as a mere, although an essential auxiliary, and might be proved yet otherwise.

LEMMA.

No triangle can have the sum of its angles greater than two right angles.

If this is proven of right angled triangles, then is it also proven of all triangles; for any triangle whatever, may be divided into two right angled triangles, whose oblique angles together constitute the angles of the given triangle; consequently if it is shown that the oblique angles of neither can together exceed a right angle, it is also shown that the angles of the given triangle cannot exceed two right angles.





Accordingly, to prove the above for right angled triangles, let ABC be a triangle, right angled at B; and, if possible, let the two angles at A and C, be together greater than a right angle. Upon AB raise the perpendicular AD equal to BC and join CD. Because the two oblique angles of the triangle exceed a right angle, while the two constituent angles of BAD are only a right angle, it follows that DAC is less than its alternate angle ACB. Reverse AC upon itself, and revolve D down to O, CO must fall within ACB, but, being only equal to BC, cannot intersect AB. Therefore the triangle ACO lies wholly within ABC, and O, or D, exceeds the right angle at B. (Euc. 1, 21.) Then the quadrilateral $ABCD$ has two right angles, and has the angles BCD, CDA, equal, and each greater than a right angle.

Revolve this quadrilateral over into the position $ABC'D'$;

then DAD' is one straight line, and CBC' is also one straight line, and the quadrilateral $CC'D'D$ has its four angles equal to one another, and each greater than a right angle. Revolve it over upon its *terminal side* $D'C'$, into the position $C'C''D''D'$, then $DD'D''$ is a reëntering angle and $CC'C''$ is another equal to it. Draw ${}_1b$ and ${}_1b$ perpendiculars to the *medial line* $D'C'$ at the apices D' and C'. They will meet CD between C and D, and they will also meet $C''D''$ between C'' and D''. Thus there will be formed a new quadrilateral ${}_1bb_1$, having its angles all equal and in excess of the corresponding angles at C, D, D'', C'', because they are exterior to them in the triangles DD'_1 $D'D''b$, etc. Also, for the like reason, and because D, D'', etc., are obtuse, the lines D'_1, $D'b$, etc., are greater, respectively, than $D'D$, $D'D''$, etc. Revolve the new quadrilateral over on its terminal side bb, into the new position $bb'b'b$. Then ${}_1bb'$, ${}_1bl'$ are reëntering angles whose apices are at the extremities of the medial line bb. Perpendicular to that line, at those extremities and apices, raise the lines $22'$, $22'$; they will meet ${}_{11}$ and $b'b'$ within the points ${}_{1,\,1}$ and b', b', respectively. And by revolving over the quadrilateral $222'2'$ upon its terminal $2'2'$, this last will become a third medial line whose extremities are the apices of a third pair of reëntering angles $22'2'$, from which a third pair of perpendiculars $2'3$, may be raised, cutting the fixed terminal side, DC, in the points 3, 3, and making 33, less than 22, or than its equal, $2'2'$. And this course of construction, it is evident, may be continued at pleasure, so long as there remains a medial line at whose extremities new perpendiculars may be raised and produced backwards, to cut both DC and its corresponding and opposite terminal side; that is to say, so long as the successive segments $C1$, $C2$, $C3$, etc., have not increased to a length, as CZ, which shall have either met its opposite and equal DZ', at the middle point between D and C, or the two shall have passed each other as seen in the figure. Now these segments cannot but so increase as to meet in the middle of DC or to cross; for the interior angles at 1, 2, 3, etc., are less than right angles, and consequently $C1$ exceeds a perpendicular from C to $C'\,1$, also 12 exceeds a perpendicular from 1 to $b2$, and so on successively. Moreover these perpendiculars themselves successively increase in magnitude, because they subtend, successively, larger angles $CC'1$, $1b2$, $22'3$, etc., and from the extremities of successively longer sides $C'1$, $b2$, $2'3$, etc., opposite the respective right angles, as is shown by the construction.* Therefore neither $C11$, 22, 3, nor any succeeding increment of CZ or DZ' can be so small as the perpendicular from C to $C'1$. Let SZ, $S'Z'$ be the two equal perpendiculars which are the first to meet or pass each other. These are drawn, by construction, from the extremities of a medial line. Thus, it follows, there can be drawn from a point, W, of their meeting and crossing, two perpendiculars to one and the same line, which is impossible. Therefore it is absurd to suppose *that any right angled triangle, or any triangle whatever, has the sum of its angles greater than two right angles;* which was the point to be proved.

PROPOSITION.

Through a given point there can be but one parallel to a given straight line.

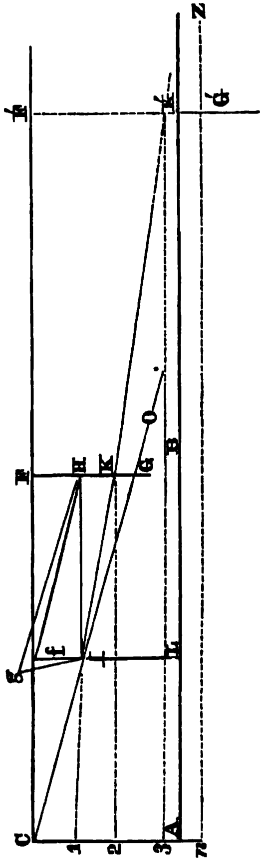
Let AB be the given straight line, and C a given point. Through C draw CF' at right angles to CA, a perpendicular to the given line. Then CF' is the only straight line through C, which cannot meet AB.

DEMONSTRATION.

Let there be any straight line, as CO, drawn through C, within the right angle at that point, and from some point G therein let GF be a perpendicular to CF'. Bisect CF in f, and raise the perpendicular fI, meeting CO in I. In FG, produced if necessary, take FK double fI; join and produce IK, and draw IH to the bisecting point between F and K. Because it has been shown that the right angled triangle $Cf1$ cannot have its three angles together greater than two right angles they must either be together equal to two right angles,

* For the reëntering angles, successively diminish, and therefore the half difference between each, and two right angles increases; also $C'1 > C'C$, and much more, $b1 > C'C$. In like manner $2'2 > b1$ and so on. And to suppose that the same or a less perpendicular can subtend the same given angle (and much more a greater angle) at a greater distance from the angular point, may easily be seen to involve the absurdity that two perpendiculars to the same line may be drawn from that angular point.

or together less than two. If the latter, then will FG be greater than FK; for let f and H be joined, and obviously the triangle fFH, by construction, is equal and similar to CfI, and therefore has the sum of its angles less than two right angles. But the triangle fIH cannot have more than two right angles, consequently the two triangles fFH, fIH, which together constitute the quadrilateral $fFHI$, cannot have so much as four right angles, and therefore the two angles fIH, FHI, are together less than two right angles. But from the equality of the perpendiculars fI, FH, the last named angles are equal to one another, and therefore each is less than a right angle. Reverse IH upon itself, and revolve over HG into the position Ig. Then, because $HIg = IHG$ exceeds a right angle, it will lie out of HIf, which has been proved less; and, because fHG, GIf, are the supplements, respectively, of the equal angles fHF, CIf, they are equal to one another; and fHG less IHG, or the angle fHI, is less than GIf diminished by HIf, or in other words, than the angle $HIG = IHg$. Consequently IHg lies outside of IHf, and the apex g lies outside of the triangle IfH, and $Ig = HG$, is greater than If, or than its equal by construction, HK; because, if f and g were joined, the angle Ifg would be greater and the angle Igf less than a right angle.



But also the angle $IgH = IGH$ is less than IfH; by which it appears that $IGH + FfH = CGF + FCG$ is less than the two angles at f within the quadrilateral; that is, they are less than a right angle. Consequently it is proven that *if the angles of CfI are together less than two right angles, the same* *is true also of the angles of CFG; and also that HG is greater than $HK=fI$.* From I drop IL a perpendicular to AB, then, the same things being given, the angle LIG, must lie within the angle LIK. And yet more, *if necessary or desired*, let the perpendicular $F'G'$ be raised at the distance $CF'=2\,CF$, and $F'K'$ be taken twice FK, then the same things given, and the line IK' being drawn, it may in like manner be shown that if CG produced will meet the perpendicular in G', $F'G'$ must be greater than twice FG, and much more be greater than twice FK, and that LIG' would lie within the angle LIK'; but if CG produced cannot meet $F'K'$ produced, then much more must the angle LIO lie within LIK'. And still again, if another perpendicular $F''K''$ be supposed, the double of $F'K'$, and raised at a supposed distance CF'' twice that of CF', and meeting CI produced in G'', then will $F''G''$ be more than twice $F'G'$, and so on indefinitely, and the same things may be proven as before.

It follows from all the above that if the line CI produced passes through K, then the three angles of CfI are not less than two right angles. But they cannot be greater and must therefore equal two right angles. And, in fact, it will readily be seen, *ex converso*, that if the angles of CfI are equal to two right angles, then CI produced must pass though K, and also through K', K'', etc., indefinitely, and must meet the line AB. To prove this join fH, then the triangle fFH by its construction is every way equal and similar to CfI, and its angle at H being complementary to its angle at f is equal to the angle HfI, which is complementary to the same,—wherefore (Euc. 1. 4.) the triangles fHF, HfI, have their third sides equal to each other and to Cf, and also the angle at I equal to the right angle at F, and also the two angles at H, or FHI, a right angle; and IF is a rectangle. Consequently, also, the two triangles IHK, CfI, having the angles at H and f right angles, and the sides HK and fI equal by construction, and the sides IH, Cf equal, are equal and similar triangles—wherefore the three angles at I together make two right angles, and CIK is one straight line, and the angles of CFK are together two right angles. And in like manner may the same be proven of the triangle $CF'K'$, and of the supposed triangle $CF''K''$, and so on indefinitely. Take in CA produced the distances $C1$, $C2$, $C3$, equal, respectively to fI, FK, $F'K'$ and so on,—each being double of the preceding, till Cn is found a greater than CA, and join $I1$, $K2$, $K'3$, and so on. In the same manner as IF was proven a rectangle may $f1$ be proven to be the same, and also, consecutively, $F2$, $F'3$, and so on indefinitely for successive perpendiculars to DA, each of which is met by CI produced, till finally the perpendicular Zn is proven to be met, as in Z, by the same. But AB cannot meet Zn, for if so it would form with it and the segment An a triangle whose angles were together greater than the two right angles. Consequently CI produced will meet AB. Hence it has been demonstrated that *if CI produced passes through K it will meet AB*; and it has before been shown that *if it does not pass through K* it will pass at some angle LIG within LIK, and consequently *much more will it meet AB.* Therefore it will meet AB whether it does pass or does not pass through K. But CI or CO is any line whatever that makes an inner angle at C with CF'. And if that inner angle is made on the opposite side from O, the line must, if produced, meet AB on that side, as already proved for the side towards O. But CF' cannot meet on either side; for, if so meeting, it would form with AB and AC a triangle having its angles together in excess of two right angles, which is absurd. Consequently CF', which cannot itself meet AB, is the only line through C that cannot so meet:—which was to be demonstrated.

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2. On the Construction of three Maps of Europe, France, and North America, in the Gnomonic Projection, with a view to the Distribution of Mineral Wealth and the study of the Earth's Figure. By Felix Foucou, Madison, Wisc.

I have the honor to present to the American Association the formula and calculations relating to a set of maps in the gnomonic projection, in the construction of which I have been engaged about four years. In such a system of projection, the surface of a country is developed upon a plane, tangent to the terrestrial spheroid at the centre of the said country; all the points of the map are determined by the intersection of that plane with the radii of the sphere. The result of it is that every grand circle is represented as a straight line. So the eye is able to discover at a glance, whether the line which connects several points of geographical or geological importance, is or is not a grand circle of the earth. The practical superiority of these maps over all others now used lies in the fact, that on the latter ones every grand circle is represented by a curve line, which can be determined only by a graphical construction, or a series of calculus operations which involve a loss of time, and sometimes chances of mistake.

Finally, the practical value of the gnomonic projection is to guide more surely the mining operations and the geographical studies, as it is well ascertained that many grand circles of the earth are, altogether, the lines of dislocation of the earth's crust and the belt of mineral wealth of similar origin. Such a relation is fully illustrated, both in Europe and America, by recent discoveries of metallic mines and mineral springs, and by the close study of the main features of the two continents. However it must be understood that I engaged myself in the construction of gnomonic maps, not in order to advocate any theory of the figure of the earth, but to collect facts and compare them more easily than it is possible to do it with the actual systems of projection; these systems are excellent for several purposes, and we must keep them; but they require a complementary one, which is the gnomonic or radiated system.

A map of Europe, about forty square feet large, is quite finished since January last, and will be engraved in order to represent the geological structure of that continent. The original sheet is in Paris (France), and will be brought to America as soon as the work of engraving is completed.

At the same time, my assistant is engaged in the drawing of a special map of France, measuring about ten square feet, and a preliminary map of North America, of about the same size.

The formulas which have been used are new and simple; they require but the knowledge of elementary mathematics, and are calculable by logarithms. The calculations are to determine the meridians by their distance from an axis, and the parallels by their intersections with each meridian. The formula relating to the meridians is

$$d = R \text{ tang } l \cos L'$$

R being the radius of the sphere, l the longitude of the meridian, L' the latitude of the centre of the map, which centre is the point of tangence of the sphere with the plane of projection; d is the distance between the same centre and the point where the meridian intersects the axes of the map, which axis is the meridian of the centre.

The formula relating to the parallels is

$$x = \frac{R \cos \lambda \cos \delta \text{ tang } l}{\sin (\lambda + \Psi)}$$

R being the radius of the sphere, l the longitude of the meridian, λ the latitude of the parallel, δ and Ψ two auxiliary angles, and x the distance between the axis of the map and the intersection of each meridian with each parallel.

The geographical drawing has been, and is still, actually performed in Paris, with the prospect of being subsequently pursued in the United States, and I will not fail to let the *American Association* know the progress of the work and its practical applications to the art of Mining and the Science of Geography.

II. MECHANICS.

1. THE LAWS OF THE DEFLECTION OF BEAMS EXPOSED TO A TRANSVERSE STRAIN, TESTED BY EXPERIMENT. By W. A. NORTON of New Haven, Conn.

I propose, on the present occasion, to communicate the principal results of a series of experiments made with an apparatus which I devised for the purpose of testing the theoretical laws of the deflection of beams exposed to a transverse strain.

[The apparatus was described in detail with the aid of several large diagrams. The following description will suffice to give an accurate idea of its essential features.]

It may be regarded as consisting of three different portions, viz.: (1) that which supports the stick to be experimented on; (2) that which applies and measures the strain; (3) that which measures the deflection produced. The supporting apparatus consists of two skeleton iron tables, each of which is a rectangular wrought iron frame, two and three-fourths feet long by two feet broad, resting at the four corners upon cast-iron legs two and a half feet high. The longitudinal bars of this frame are one inch broad by two inches deep. These tables are placed lengthwise on the same line, say a north and south line, and one foot four and a half inches apart. Each of them supports a transverse sliding frame composed of two wrought iron bars, one inch by two inches, placed four inches apart, and formed at the ends into two sliding saddle pieces that rest upon the longitudinal bars of the table-frame. These sliding frames can be set at any distance apart, from two feet to six feet. Upon each of them rests an iron plate five-eighths of an inch thick, and fitted by grooves to the two iron bars, so as to be movable in the direction of their length, or crosswise to the lengths of the tables. Upon these plates rest two cast-iron supports, each consisting of an upright pillar, one and one-eighth inches square in cross section and twelve inches high, connected at the bottom with a plate that is supported by four leveling screws upon the sliding plates just described, and at the top with a plate five inches long (crosswise to the table-frames), two and one-eighth inches broad and five-eighths of an inch thick. The nearer edges of the top plates of these upright supports are beveled off, so as bring them immediately over the pillars. The stick experimented on rests immediately on these iron plates, and its effective length is the distance between their nearer edges.

The mechanical contrivance for applying the strain to the stick, like the supporting apparatus just described, is wholly made of iron, and consists of an upright screw, turning by means of female screws in two horizontal plates fastened at their four corners to upright columns. These columns are firmly connected with an iron bed plate, which is placed between the table supports above described and securely fastened to the floor-joists by long screw-bolts. The screw-head is connected by intermediate pieces with an iron stirrup that rests crosswise upon the stick. There is a special arrangement, which cannot well be described here, by which these intermediate pieces are made to move in a truly vertical direction, and not partake, in any degree, of the revolving motion of the screw. One of the intermediate pieces referred to, is a Fairbank's Spring Dynamometer (essentially the same as Regnier's). The circular dial-plate reads from one pound to one thousand pounds. By means of the screw a power of one thousand pounds can be applied to the stick; but in the experiments the strain was in no instance carried higher than five hundred pounds.

The apparatus for measuring the deflection produced, consists of a brass lever of two arms, each five inches long, one end of which is depressed by the middle of the bent stick, and the equal rise of the other is measured by a micrometer-screw. This micrometer-screw reads to one ten-thousandth of an inch. The lever is placed crosswise to the length of the stick and opposite its middle. It passes through a vertical slot in the iron stirrup that rests upon the middle of the stick, and presses by a blunt steel knob against the under side, the farther end of the lever being made slightly heavier than the other, so as to secure a moderate pressure. The arrangement for supporting the lever consists of a wooden strip six and a half feet long, two and three-eighths inches deep, and one-half inch thick, stiffened along the top by a strip of brass. This is secured to the pillars of the two upright supports, by clamping pieces, which firmly hold it at a distance of five inches from the centres of the pillars and parallel to the length of the stick. Upon this supporting strip rests a sliding saddle-piece, having a small flat plate on the top, and adjustable by horizontal screws that pass through its vertical side plates and press against the vertical sides of the wooden strip. Upon the top plate of this saddle-piece rests, by means of four leveling screws, another small plate, upon which the knife-edge of the lever immediately rests. From the vertical side-plate of the saddle-piece that is farthest from the stick, extends a small horizontal bar, six inches long. This lies directly under the farther half of the lever. Near its farther end the micrometer-screw passes through it from below upward, and touches the under side of the lever, at a distance of five inches from the knife-edge support. The contact of its rounded point with the lever is observed with a microscope. The screw-head, adapted to the lower end of the screw, is two inches in diameter. Its outer vertical edge is silvered, and graduated to read to thousandths of an inch; but a small vertical wire fastened to the bar above it, past which the screw-head moves, subdivides the smallest space on the graduation, so as to make it possible to read to the one ten-thousandth of an inch. Since the knife-edge of the lever is at the same distance from the point of contact with the upper end of the micrometer-screw and from that with the middle of the under side of the stick, the micrometer readings are the linear deflections of the middle of the stick. It will be observed that the depressions of the knob of the lever under the middle of the stick, will be the actual deflections; since the support of the fulcrum of the lever is firmly connected with the upright supports of the stick, and will partake of any depression that they may experience from any settling of the apparatus under the action of the force that produces the deflection. It may be remarked here that it was ascertained, by an independent set of experiments, that the actual depression of the apparatus was very small; only from two to three-thousandths of an inch for every one hundred pounds of pressure.

The manner of conducting the experiments will be readily understood. The inner edges of the top surfaces of the upright supports upon which the stick is to rest, are set at a distance apart equal to the proposed effective length of the stick; either two feet, four feet, or six feet. These surfaces are carefully leveled and adjusted to the same horizontal plane. The lower plates of the upright supports are firmly clamped to the sliding frames on which they rest, by portable clamps. The stick is then put in its place, the iron stirrup placed crosswise upon its middle, and connected by a sliding bolt and key with the upper end of the closed spring of the dynanometer. The apparatus for measuring the deflections is also leveled and adjusted. The screw is then slowly turned until the index of the dynanometer indicates one hundred pounds, two hundred pounds, or any other force of pressure it is proposed to apply, and the reading of the micrometer-screw indicates the linear deflection of the middle of the stick produced by the pressure applied.

The experiments were made upon white pine sticks of various lengths, from two feet to six feet, and various breadths and depths, from one inch to four inches. The results are derived from the means of a large number of experiments. As an additional test the experiments were repeated upon a second set of sticks.

The received theoretical formula for the deflection of beams of a rectangular cross section of uniform dimensions, is $f = m \frac{Pl^3}{Ebd^3}$ in which m is a constant, P the power applied, E the modulus of elasticity, l the length, b the breadth, and d the depth of the stick. For the case of a beam resting freely on two supports and loaded in the middle, to which the experiments were entirely confined, this becomes $f = \frac{Pl^3}{4Ebd^3}$. If this formula be correct, then the following laws must be true.

(1). The deflection is directly proportional to the pressure.

(2). It is inversely proportional to the breadth.

(3). It is inversely proportional to the cube of the depth.

(4). It is directly proportional to the cube of the length.

We will now compare each of these laws with the experimental results obtained.

First Law. The deflection is directly proportional to the pressure. The following table contains a few of the results that serve to test this law. The first three columns give the lengths, breadths, and depths of the sticks, and the last column gives the differences between the deflections produced by the two pressures given in the column headed Difference of Pressures.

The deflections for the first one hundred pounds of pressure are not included, for the reason that there is more liability to error in observing the absolute deflection than in observing the increments of deflection produced by each one hundred pounds of augmented pressure, starting from a pressure of one hundred pounds. The actual results for the first one hundred pounds are given, for comparison, at the bottom of the table.

TABLE I.

Sticks.			Diff. of Pressures.	Diff. of Deflections.
Length.	Breadth.	Depth.		
				in.
2 ft.	4 in.	2 in.	100 lbs. to 200 lbs.	0.0114
"	"	"	200 " " 300 "	0.0102
"	"	"	300 " " 400 "	0.0090
"	"	"	400 " " 500 "	0.0090
4 ft.	2 in.	3 in.	100 " " 200 "	0.0459
"	"	"	200 " " 300 "	0.0460
2 ft.	3 in.	2 in.	100 " " 200 "	0.0159
"	"	"	200 " " 300 "	0.0135
"	"	"	300 " " 400 "	0.0127
2 ft.	2 in.	3 in.	100 " " 200 "	0.0088
"	"	"	200 " " 300 "	0.0084
"	"	"	300 " " 400 "	0.0080
				in.
2 ft.	4 in.	2 in.	0 lbs. to 100 lbs.	0.0119
4 "	2 "	3 "	" " "	0.0477
2 "	3 "	2 "	" " "	0.0164
2 "	2 "	3 "	" " "	0.0092

The following table gives the deflections for one hundred pounds, two hundred pounds, three hundred pounds, &c., pressure:

TABLE II.

Sticks.			Pressures.	Deflections.
Length.	Breadth.	Depth.		
				in.
2 ft.	4 in.	2 in.	100 lbs.	0.0119
"	"	"	200 "	0.0233
"	"	"	300 "	0.0335
"	"	"	400 "	0.0426
"	"	"	500 "	0.0516
4 ft.	2 in.	3 in.	100 "	0.0477
"	"	"	200 "	0.0936
"	"	"	300 "	0.1396
2 ft.	3 in.	2 in.	100 "	0.0164
"	"	"	200 "	0.0323
"	"	"	300 "	0.0458
"	"	"	400 "	0.0585
2 ft.	2 in.	3 in.	100 "	0.0092
"	"	"	200 "	0.0181
"	"	"	300 "	0.0264
"	"	"	400 "	0.0344

It appears from the results given in tables I. and II, that the deflection is approximately proportional to the pressure; but strictly speaking increases according to a less rapid law. The probable explanation of this descrepancy between theory and fact, is that as the force of pressure increases the neutral axis of the cross section of the stick shifts its position, and its distance from the centre of gravity of the cross section augments as the pressure becomes greater. From this cause the moment of the resistance to flexure increases indirectly with the pressure, at the same time that it increases directly from the augmented strains of the fibres. The increased moment of resistance to flexure resulting from this shifting of the neutral axis, should be attended with a diminished increment of deflection for the same increment of pressure.

Second Law. The deflection is inversely proportional to the breadth. Table III. will serve to test this.

TABLE III.

Sticks.			Obs. Deflection for 100 lbs.	Cal. Deflection for 100 lbs.	Difference.	Ratios of Error.
Length.	Br'dth.	Depth.	in.	in.	in.	
2 ft.	1 in.	2 in.	0.0423	0.0423		
"	2 "	"	0.0195	0.0211	+0.0016	1-12
"	3 "	"	0.0147	0.0141	−0.0006	1-24
"	4 "	"	0.0106	0.0106	0.0000	
4 ft.	1 "	"	0.2858	0.2858		
"	2 "	"	0.1200	0.1429	+0.0229	1-5
"	3 "	"	0.0983	0.0952	−0.0031	1-32
"	4 "	"	0.0624	0.0714	+0.0090	1-7

The numbers in the column of calculated deflections are obtained by assuming the observed deflection for the smallest breadth (one inch), and computing the deflection for the other breadths on the supposition that it is inversely proportional to the breadth. The last column gives the ratios of the differences between the observed and calculated deflections, given in the preceding column, to the observed deflections. The observed deflections answer to an increase of pressure from one hundred pounds to two hundred pounds, or two hundred pounds to three hundred pounds. It will be seen that the errors are some plus and others minus, and that the ratios of error are small fractions. They are, however, too great to be attributed entirely to errors of observation; but not greater than may reasonably be ascribed to differences in the moduli of elasticity of the different sticks, and to the greater shifting of the neutral axis in the case of the sticks most strained, in connection with possible errors of observation.

Third Law. The deflection is inversely proportional to the cube of the depth.

It soon became evident, in the course of the experiments, that this law could not be regarded as even approximately true, except in the cases of sticks, or beams, whose length bore a high proportion to their depth. The following comparisons of observed with calculated deflections will show that it fails in the case of sticks two feet in length.

TABLE IV.

Sticks.			Ratios of bd^3	Obs. Deflec.	Cal. Deflec.	Diff.	Ratios of Error.
Length.	Br'dth.	Depth.					
				in.	in.	in.	
2 ft.	2 in.	1 in.		0.1414			
2 "	1 "	2 "	1 to 4	0.0423	0.0353	—0.0070	1·6
2 "	3 "	2 "		0.0147			
2 "	2 "	3 "	1 to 2¼	0.0084	0.0065	—0.0019	1·4.4

TABLE V.

Sticks.			Ratios of d^3	Obs. Deflec.	Cal. Deflec.	Diff.	Ratios of Error.
Length.	Br'dth.	Depth.					
				in.	in.		
2 ft.	2 in.	1 in.		0.1414			
"	"	2 "	1 to 8	0.0195	0.0177	—0.0018	1·11
"	"	3 "	1 to 27	0.0084	0.0052	—0.0032	1·2.6

The calculated are all less than the observed deflections. The same is true when sticks of greater length than two feet are taken, but the errors are smaller in proportion as the length is greater. It appears, therefore, that the deflection decreases according to a less rapid law than the inverse cube of the depth.

Fourth Law. The deflection is directly proportional to the cube of the length.

The experiments show that this law fails, as well as the third.

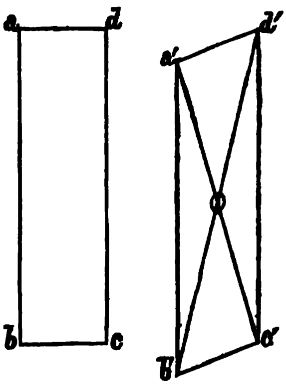
TABLE VI.

Sticks.			Ratios of l^3	Obs. Deflec.	Cal. Deflec.	Diff.	Ratios of Error.
Length.	Br'dth.	Depth.					
				in.			
2 ft.	2 in.	2 in.		0.0195	in.	in.	
4 "	2 "	2 "	1 to 8	0.1200	0.1560	+0.0360	1-3.33
2 "	2 "	3 "		0.0084			
4 "	2 "	3 "	1 to 8	0.0460	0.0672	+0.0212	1-2
2 "	3 "	2 "		0.0147			
4 "	3 "	2 "	1 to 8	0.0983	0.1176	+0.0193	1-5
2 "	4 "	2 "		0.0106			
4 "	4 "	2 "	1 to 8	0.0624	0.0848	+0.0224	1-2.8

We may conclude, from these results, that the deflection increases according to a less rapid law than the cube of the length of the stick. We have already seen that it decreases in a less rapid proportion than the inverse cube of the depth. It follows, therefore, that the true formula for the deflection probably contains at least one additional term, which varies less rapidly than as the cube of the length directly and the cube of the depth inversely; or in other words, contains l in the numerator, and d in the denominator, each raised to a lower power than the cube. Now, if we consider attentively the changes that must occur in the relative positions of the molecules within a stick or beam, when it is subjected to a cross strain, we may perceive that a cause of deflection exists which has hitherto been disregarded, or deemed too insignificant in its effects to be taken into account.

It is plain that when a stick, or beam, of a uniform rectangular cross section, resting on two supports, is loaded at its middle, a vertical force equal to one half the weight is transmitted to each support, by the slipping of each vertical section, or lamina, upon the next, until a vertical force of resistance is called into play equal to half the weight. As each section must transmit this same force to the next, the slipping of one section upon another must be the same from the middle to the end. The section at the middle will be directly depressed, from this cause, by an amount equal to the sum of all these displacements that occur between the middle and the end, or to any one displacement multiplied by the number of sections, or indefinitely thin laminæ, in this interval. This depression should, then, be directly proportional to the half-length, or length of the stick. If we compare sticks of the same length but different cross sections, the number of fibres that are subjected to this cross strain, or the number of material points in each cross section whose vertical resistance will be called into operation by the slipping of this section upon the next, will be proportional to the area of the cross section, and hence the amount of the relative sliding displacement will be inversely proportional to this area. It will be seen, then, that the sinking, or linear deflection of the middle of the beam, thus directly resulting from the slipping of contiguous vertical sections, may be represented by the expression $c \frac{Pl}{bd}$; in which P is the load, l the length, b the breadth, and d the depth of the beam, and C a constant that must be determined by experiment.

The theoretical deflection given by the formula which has been under discussion, is due to longitudinal strains on the fibres, indirectly resulting from the same slipping of contiguous sections. If *ab* and *cd* represent two vertical cross sections, indefinitely near to each other, of which *ab* is nearest the middle of the beam, the transmission of the action of the half-weight will cause *ab* to slip relatively to *cd* until a vertical resisting force comes into operation equal to the half-weight; and the rectangle *abcd* will take the form of the oblique parallelogram, $a'b'c'd'$. The diagonal *ac* will therefore be shortened, and *bd* lengthened. Accordingly a strain of compression



will come into operation along $a'c'$, and a strain of extension along $b'd'$. The reactions to these strains will take place, along $a'c'$ from the middle o toward a', and from o toward c', and along $b'd'$ from b toward o, and from d' toward o. As a consequence the points a', and d', will be urged by equal forces toward the left, or toward the middle of the beam, and the points b', c', by the same forces, toward the right. The sum of the first two forces will then be a longitudinal strain of compression on the upper fibre, and the sum of the last two an equal strain of extension on the lower fibre. It will be observed that the strains here considered as confined to the upper and lower fibres, are actually distributed over all the fibres above and below the middle fibre of the beam.

The conclusions here arrived at with regard to one indefinitely small rectangle *abcd*, will be equally true of any other that may be considered between the middle and end of the beam; and the same longitudinal strains will be developed by the slipping of contiguous sections, in each rectangle. The entire longitudinal strains on the fibres, at the middle, will then be the sum of the individual strains developed in all the rectangles of the half length of the beam. The ordinary equation of equilibrium of a beam may be readily made out from the present point of view; but we have now only to consider the matter of deflection. It will be seen that the movements, to the left and right, of the angular points of the parallelogram $a'b'c'd'$, that have been signalized, will be attended with a turning of the whole parallelogram from right to left around its centre o. The direct tendency to rotation will be the same for each parallelogram of the half length of the beam, but owing to the propagation of the longitudinal strains on the fibres developed at each parallelogram, from the end to the middle, the actual compressions and elongations will be greatest at the middle, and the actual rotation of the parallelogram there will be the greatest. The deflection consequent upon the elongations and compressions of the fibres, is the joint result of the rotary movements of all the parallelograms in the half length of the beam; and it is represented by the formula which has been under discussion.

It would seem, then, that the true theory of deflection con ducts to the following formula, in the special case of a beam resting on two supports and loaded in the middle.

$$f = c\frac{Pl}{bd} + \frac{Pl^3}{4Ebd^3}$$

Let us now proceed to compare this formula with our experimental results. For this purpose we will determine the values of the two constants C and E for each individual stick, and compare the several values obtained. If the formula be correct, the different sticks all in precisely the same mechanical condition, and the experiments perfectly accurate, we should get the same values for these constants in the case of each stick. But the experiments are liable to more or less of error, and the sticks may differ materially from one another in their mechanical condition; and even where they do not, as the actual deflections experienced are so different with sticks of different dimensions, any changes in the values of the constants that may result from the shifting of the position of the neutral axis, under the operation of the strains, should differ more or less. The derived values of the constants may therefore differ among themselves, within certain limits, without leading us to conclude that the formula is probably at fault.

The following table contains the values of C and E, calculated from the deflections due to one hundred pounds, for two sets of sticks of the same dimensions. They were all over four feet in actual length, but in the experiments the effective lengths taken were either two feet or four feet. The values of E and C were obtained, in some instances, by taking the deflections for the same breadth and depth but different lengths (either two feet or four feet), and in other instances by taking the deflections for the same length but different breadths and depths. The results of two sets of calculations are given in the table. In the one the deflections answering to the least and greatest strains are taken, and the deflection due to one hundred pounds computed from the difference of these by simple proportion; in the other the same is obtained by taking the deflections answering to strains, or pressures, intermediate between the extreme strains.

TABLE VII.

Sticks.			Diff. of Extreme Pressures.		Diff. of Intermediate Pressures.	
Set No. 1.			*E.*	*C.*	*E.*	*C.*
l.	*b.*	*d.*				
ft. ft.	in. in.	in. in.				
2, or 4	2	1	1,359,500 lbs.	0.0000108	1,308,430 lbs.	0.0000082
2	2, or 3	3, or 2	1,566,809 "	0.0000100	1,579,960 "	0.0000095
4	2, or 3	3, or 2	1,584,820 "	0.0000067	1,560,800 "	0.0000078
2, or 4	4	2	1,553,000 "	0.0000140	1,501,200 "	0.0000127
2, or 4	2	2	1,481,800 "	0.0000108	1,423,600 "	0.0000084
Means, ...			1,508,988 "	0.0000108	1,474,798 "	0.0000096
Set No. 2.						
ft. ft.	in.	in.				
2, or 4	3	2	1,377,729 lbs.	0.0000084	1,354,000 lbs.	0.0000080
2, or 4	2	3	1,395,984 "	0.0000069	1,315,000 "	0.0000068
2, or 4	4	2	1,558,900 "	0.0000110	1,542,860 "	0.0000107
2, or 4	2	2	1,561,822 "	0.0000084	1,600,000 "	0.0000100
Means, ...			1,423,600 "	0.0000092	1,427,965 "	0.0000094

The general formula applicable to white pine sticks of the general quality used in these experiments, will be obtained by taking the mean of the several values of E and C given in the above table. To test the theoretical formula we have obtained we will take the mean values of E and C, for the second set of sticks, given at the bottom of the fourth and fifth columns, viz.: $E = 1{,}427{,}965$ pounds, and $c = 0.0000094$. We thus have

$$f = 0.0000094 \frac{Pl}{bd} + \frac{Pl^3}{5{,}711{,}860 \times bd^3}$$

or, taking $P = 100$ lbs.,

$$f = 0.00094 \frac{l}{bd} + \frac{l^3}{57{,}118.6 \times bd^3}$$

The following table contains the values of f calculated by this formula, and the results of a comparison of the calculated values with the deflections observed.

TABLE VIII.

Sticks.			Cal. Values of f.	Obs. Deflec.	Difference.	Ratios of Error.
l.	b.	d.	in.	in.	in.	
4 ft.	3 in.	2 in.	0.0882	0.0983	—0.0101	1-9.7
2 "	3 "	2 "	0.0140	0.0147	—0.0007	1-21
4 "	2 "	3 "	0.0434	0.0460	—0.0026	1-18
2 "	2 "	3 "	0.0082	0.0084	—0.0002	1-42
4 "	4 "	2 "	0.0661	0.0624	+0.0037	1-17
2 "	4 "	2 "	0.0104	0.0090	+0.0014	1-6.4
4 "	2 "	2 "	0.1323	0.1200	+0.0123	1-9.8
2 "	2 "	2 "	0.0208	0.0195	+0.0013	1-15

The comparative accuracy of the old and new formulas, will be seen on comparing the ratios of error in tables IV, V, and VI, and table VIII. It should be added that the results given in table IV, are from calculations made in each instance on sticks which are identically the same, whereas those in table VIII, are affected with the errors resulting from the fact that the values of E and C are the mean values obtained from a number of different sticks, which may differ more or less in their mechanical condition. If we take the average values of these constants given in the table for the stick three inches by two inches in cross section, and obtained by taking the deflections answering to the lengths, two feet and four feet, the formula thus obtained gives, for these transverse dimensions and the lengths, two feet and four feet, respectively, four results, the ratios of error of which lie between one fifteen-hundredth and one one-hundred and twentieth.

Let us compare the values of the first and second terms of the formula. This is done in table IX. The values of E and C taken in the formula, are the individual values for each stick given in the second and third columns of table VII.

TABLE IX.

Sticks.			First Term.	Second Term.	Ratio.	Sum.
l.	*b.*	*d.*	in.	in.		in.
4 ft.	3 in.	2 in.	0.00692	0.08952	1-12	0.0964
"	2 "	3 "	0.00692	0.03979	1-5.7	0.0467
"	4 "	2 "	0.00658	0.0554	1-8	0.0620
"	2 "	2 "	0.01000	0.1106	1-11	0.1206
"	1 "	2 "	0.01776	0.2681	1-15	0.2858
2	3 "	2 "	0.00346	0.01119	1-3.2	0.0146
"	2 "	3 "	0.00346	0.00497	1-1.4	0.0084
"	4 "	2 "	0.00328	0.00693	1-2	0.0102
"	2 "	2 "	0.00500	0.01383	1-2.8	0.0188
"	2 "	1 "	0.00990	0.13150	1-13	0.1414
"	1 "	2 "	0.00990	0.03301	1-3.3	0.0429

It will be observed that the value of the first term is in general comparatively larger for the length of two feet, than for that of four feet; and in two instances is as large as one-half the second term.

If now we divide the first term by the second, we obtain as the general expression of their ratio, $4\,E\,C\frac{d^2}{l^2}$; from which we see that it is proportional to $(\frac{d}{l})^2$. When $\frac{l}{d} = \sqrt{4\,E\,C}$, $4\,E\,C\frac{d^2}{l^2}$ becomes equal to unity, and the first term equal to the second. When $\frac{l}{d}$ has a less value than this, the first term is greater than the second. Taking the mean values of E and C, given in the last two columns of table VII, for the first set of sticks, we have $\sqrt{4\,E\,C} = 7.41$. The mean values given in the same columns for the second set of sticks, give $\sqrt{4\,E\,C} =$ 7.33. If therefore the length of a white pine stick be less than about seven and one-third times the depth, the deflection from the cause heretofore neglected becomes greater than from the cause to which the whole deflection has hitherto been ascribed. When the length and depth are equal, it is nearly fifty-five times greater; from which it appears that in this case the deflection directly due to the slipping of contiguous vertical laminæ, so greatly preponderates over that indirectly resulting from the same by reason of the longitudinal strains communicated to the fibres, that the latter is comparatively inappreciable.

It will be seen, from the general expression for the ratio of the two terms, above obtained, that the formula for the deflection may take the following form:

$$f = \frac{Pl^3}{4\,Ebd^3}\left(4\,EC\frac{d^2}{l^2} + 1\right).$$

I have made, with the same apparatus, a series of experiments on the degree of set, or residual deflection, communicated to sticks by varied strains, and under various circumstances, and obtained interesting and valuable results. The discussion of these experiments is reserved for another occasion.

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2. Suggestions on the Theory of the Composition of Forces. By F. W. Bardwell, of Washington, D. C.

(ABSTRACT.)

THAT velocity is proportional to the force causing it, we can only know from experience, but the principle of the composition of velocities, known as the parallelogram of velocities, is obvious from geometrical considerations, and is entirely independent of any relation between velocity and the force causing it. But if force is proportional to velocity, then the principle of the parallelogram of forces necessarily follows. If force is not proportional to velocity, then, as necessarily, the principle of the parallelogram of forces fails to hold.

The logical order of the exposition of the composition of forces then seems to be:—First, to establish from geometrical considerations the parallelogram of velocities.—Secondly, to show that in all the varied combinations of forces, acting both in nature and under the control of man, the results verify the

assumption that force is proportional to velocity, and that this principle may be accepted.—Thirdly, that the principle of the parallelogram of forces necessarily results from the two previous.

LaPlace, in his *Mecanique Celèste*, reverses this order, and deduces the parallelogram of velocities from the parallelogram of forces. In his demonstration, however, he assumes that if two component forces, x and y, become successively dx, $2dx$, $3dx$, etc., and dy, $2dy$, $3dy$, etc., that their resultant z would become successively dz, $2dz$, $3dz$, etc., and the angle 0 between the directions of x and z would remain constant. But it seems to me that these results can only be considered to take place, on the hypothesis that force is proportional to velocity, and that they necessarily rest upon it. LaPlace does not admit this hypothesis until at a subsequent point of his investigation, and therefore his demonstration has that fallacy.

Lagrange says that Bernoulli attempted to establish the principle of the composition of forces on considerations independent of that of motion, and gave a complicated demonstration based on two "principles," but these *principles* also depend on that of *force proportional to velocity*, and it would seem that the parallelogram of forces involves essentially this principle, and cannot be freed from it.

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3. On the Thermo-dynamics of Water-falls. By Alfred M. Mayer, of South Bethlehem, Penn.

Every one standing before a cataract is impressed with the presence of power in the plunging water; and those who are accustomed to consider the evolution and transmutations of force naturally inquire into what phases of motion this falling mass is converted.

The cataract leaps the brow of the precipice and strikes the water below; vibrations are generated in the air, in the earth and water, and there arises a cloud of mist from the base of the falls. These are the immediately evident results of the falling water; but yet another effect we should naturally expect to find, that is, the heating of the water after its impact on the rocks and river beneath.

This effect has been suspected by all natural philosophers; but, as far as I know, no one attempted to detect and measure it until Prof. Joseph Henry made some thermometric observations at the Falls of Niagara in Sept., 1857. Prof. Henry gave a verbal account of his observations before the Baltimore meeting of the American Association for the Advancement of Science, in May, 1858; but unfortunately they were not presented for publication in the proceedings, and all that appears in the published account of that meeting is merely the following title: "Observations made at the Falls of Niagara, 1st September, 1857, by Prof. Joseph Henry." According to my recollection of the remarks of Prof. Henry, he did not detect any difference of temperature in the water at the top and bottom of the falls. This was probably owing to the want of delicacy in the thermometer, which read only to 1° Fahr., the fractions of degree being estimated by the eye aided by a lens.

I therefore, during a recent visit to Niagara and Trenton Falls, determined to repeat these observations with a delicate Centigrade thermometer, by Alvergniat of Paris, reading to 0°.02 Cent.

The first observations were made at Trenton Falls, July 4; the others at Niagara, July 7, 1869.

The water, at each place, was collected in a tin cylinder, attached to a strong cord, holding about a half-gallon and of sufficient depth to allow the thermometer to be entirely immersed; while the temperatures were read as soon as the mercurial column ceased to descend.

TRENTON FALLS, July 4, 1869.

Sky overcast. Drizzling. No perceptible wind. Temperature of air during observations 18°. 3, C.

The top-water was taken within ten feet of the leap of the falls; the bottom-water at about thirty feet from the bases of the two cataracts; and about five minutes elapsed between the last reading of the temperature of the bottom-water and the first reading of the top-water.

Sherman Falls (Trenton), W. side. Height 39 feet.

OBSERVATIONS.	TEMP. TOP-WATER.	TEMP. BOTTOM-WATER.
1	16°.6 C.	16°.7 C.
2	16°.6	16°.7
3	16°.6	16°.7

High Fall (Trenton) upper fall W. side. Height 45 feet.

OBSERVATIONS.	TEMP. TOP-WATER.	TEMP. BOTTOM-WATER.
1	16°.6 C.	16°.7 C.
2	16°.6	16°.7
3	16°.6	16°.7
4	16°.6	16°.7

At first sight these observations seem to have established an interesting physical fact in accordance with the thermo-dynamic theory; but the *same* degree of heating (0°.1 C) in each case, and the *amount* of the heating of the water does not agree with that which Joules' unit establishes; according to which the increase in temperature at the base of Sherman Fall should have been 0°.028 C, and at the High Fall 0°.032 C.

NIAGARA FALLS, July 7, 1869.

W. side of Canada fall. Height 158 feet. Sky 0.7, cloudy; sun appearing at intervals. Wind S.; gentle breeze.

The top-water was here collected from an overhanging log, about sixty feet from the brow of the cataract and about thirty feet from the shore. The bottom-water was taken in first series of observations about two hundred feet from W. side of the base of fall, and in second series at about five hundred feet distant from base of cataract, to both of which points a strong current set in directly from the apex of "the horse-shoe," and the water when dipped was white with the air-bubbles it contained.

Niagara Falls. Observations at Top of Cataract.

OBSERVATIONS.	TEMP. TOP-WATER.	TIME OF OBSERVATION.
1	18°.85 C.	1.45 P.M.
2	18°.80	1.49
3	18°.80	1.52
4	18°.75	2.00
Mean Temp., . . .	18°.80	

Temperature of the air in shade with N. exposure, at top of the fall, during the observations, was 24°.00 C'.

Niagara Falls. First series. Bottom-water.

OBSERVATIONS.	TEMP. BOTTOM-WATER.	TIME OF OBSERVATION.
1	18°.6 C.	2.15 P.M.
2	18°.6	2.30
Mean Temp., . . .	18°.6.	

Niagara Falls. Second series. Bottom-water.

OBSERVATIONS.	TEMP. BOTTOM-WATER.	TIME OF OBSERVATION.
1	18°.55 C.	2.40 P.M.
2	18°.60	2.43
3	18°.55	2.50
Mean Temp., . .	18°.566 C.	

Mean Temp. of first and second series, 18°.583.

Mean Temperature of air at bottom of cataract, from five observations, was 22°.16 C.

The above observations show that the water after having reached the point at the base of the cataract where it was collected was *cooled* 0°.217 C, instead of being *heated* 0°.113 C, as should have taken place had all of the falling force of the water been converted into heat.

Thus at Trenton Falls we obtain a + and at Niagara a — departure from the deductions of a well established theory. In order to appreciate these apparently anomalous results we should consider the physical conditions of the water during and after the fall.

As the water approaches the brow of the precipice, and just after it makes the leap, it has different velocities at various points of section at right angles to its surface, the velocity of its particles decreasing with the depth from the surface owing to the friction of the lower strata on the river-bed. This difference of velocity first tends to break up the sheet and then the resistance of the air (which increases with the square of the velocity of the falling particles) further disintegrates the liquid; and when the sheet is thin, and the fall high, it will be completely "*atomized*," as we see in the Falls of the Staubach and of Yosemite. As the fall strikes the river below it forces under its surface a large quantity of air, and thus during and after its fall the water is under conditions most favorable for its evaporation.

When the surrounding atmosphere has a temperature above that of the top-water it will give up part of its heat when it is forced under the surface of the river below, and if the sheet be unbroken (as was nearly the case at Trenton Falls) this heating cause will be added to the heating effect of the impact. While if the sheet is disintegrated during a high fall (as at Niagara) the cooling by evaporation may equal and even surpass the heating produced by these two causes. It therefore follows that with the same temperature of top-water different temperatures will be obtained at the foot of the same fall, according to the various hygrometric and barometric conditions of the atmosphere.

Now, if we suppose that the water falls in an unbroken sheet of considerable thickness, that no vibrations of air, water and earth are produced, and that no heat is given by the atmosphere or abstracted by evaporation, we will have the total falling force converted into heat, and that easily detected with a thermometer as delicate as that used in these observations. The heating effect would be in proportion to the height of the cataract or, what is the same, as the square of the velocity of impact; a fall of seven hundred and seventy-two feet being requisite (according to the experimental determinations of Joule) to heat the water 1° Fahr., which converted into centigrade equivalent gives $772 \times \frac{9}{5} = 1390$ feet of fall for an elevation of 1°, Centigrade.

Thus we see why it was that at Trenton we obtained a heating effect of 0°.1 C., while theory gives 0°.032 C. (for the mean of the two falls), while at Niagara we obtained a *cooling* effect of 0°.217 C, instead of 0°.113 C. of heating; showing that the water was really *cooled by evaporation* $0°.217 + 0°.113 = 0°.33$ C., without considering the abstraction, by the same cause, of the heat imparted to the water by the air, which must have been considerable as its temperature exceeded that of the top-water by 5°.2 C.

We infer from our observations that one cannot derive from thermometric observations on water-falls any data of value to the thermo-dynamic theory; but we would suggest that observations be made when the temperatures of the air and of the top-water are alike, while at the same time the air is saturated with moisture; under these conditions the heating of the water due to its impact will be the least hidden by the heating or cooling effect of contiguous air, or by the cooling produced by evaporation. Observations should also be made under very different thermometric, hygrometric and barometric conditions of the atmosphere, which would give approximations to the measure of their several effects.

We have no doubt that the results which will be obtained under circumstances when these extraneous disturbing causes are at a minimum, will give a residual heating effect due to the impact of the falling water, and its determination under these conditions is worthy of the attention of any one who has the advantage of proximity to the falls and the leisure and patience to make the observations.

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4. Physical Theory of the Principle of the Lever. By W. A. Norton, of New Haven, Conn.

If it be true that two forces acting upon a lever will hold each other in equilibrio if their intensities be inversely proportional to their lever arms, it is plain that this principle must be a consequence of the law or laws, of the lateral transmission of force from molecule to molecule of the lever, and therefore of one or more fundamental principles of molecular action consequent upon the disturbance of the natural equilibrium of the molecules. I propose to show that it may in fact be deduced from two admitted principles of molecular action. These are:

(1) If two integrant molecules of a solid body, which lie within the range of reciprocal action, be forcibly separated from each other a minute distance, a mutual attraction or repulsion will be brought into operation, and if they be urged

nearer to each other by an equal minute distance an equal opposite force of repulsion or attraction will come into play.

(2) The intensities of the forces thus originating are proportional to the amount of the relative displacement of the two molecules, on the line connecting them.

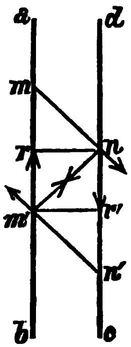
To these fundamental principles are to be added that of the parallelogram of forces, as applicable to the case of two forces acting *directly* upon the same point.

The principle of the lever presents three distinct cases, which require separate consideration.

1. *The Straight Lever, with perpendicular forces.*
2. *The Straight Lever, with oblique forces.*
3. *The Bent Lever.*

Case i. *The Straight Lever, with perpendicular forces.* We will first take the lever of the first order, and consider the precise process of transmission of either of the extraneous forces acting upon it from the point of application to the fulcrum. Let *ab*, Fig. 1, represent a vertical line of particles of one cross section, or lamina, and *cd* the contiguous vertical line of particles of the next section; and let us conceive that all the similarly situated pairs of lines of the two cross sections or laminæ, are concentrated upon *ab* and *cd*, so that these lines may represent the entire cross sections.

Fig. 1.

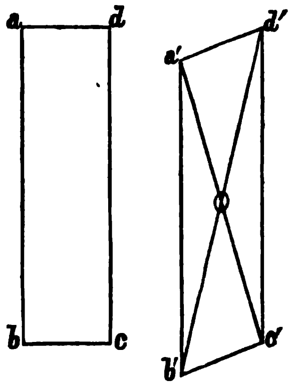


Suppose that the extraneous force is directly applied to the first line, depressing it by a small amount. If we take one particle *m'* of the first line and consider the actions upon it of two particles *n*, *n'*, of the second line at equal distances above and below it, it will be seen that *m'* will recede from *n* a minute distance, and approach *n'* by sensibly the same distance, and that the molecular forces brought into operation by these relative displacements will be opposite in their character, and equal in intensity. Thus, if the recess of *m'* from *n* develops a mutual attraction, the approach of *m'* to *n'* will develop an equal repulsion. The resultant of these two forces acting on *m'* will be directed upward, or from *m'* toward *a*. A similar result will be obtained for each pair of particles, *n* and *n′*, that exercise a sensible action on *m′;* except that for those situated beyond a certain distance the forces developed, and consequently their resultants, will be reversed, or *m′* will be urged downward by the actions of such particles. Since *m′* is held in equilibrium in opposition to the extraneous force urging the section *ab* downward, the entire resultant of all the actions of the pairs of particles *n*, *n′*, of the section *cd*, on *m′* will be directed upward. The section *ab* slips upon *cd*, under the action of the extraneous force until this resultant is equal to the extraneous force. By our second fundamental principle the amount of this slipping will be proportional to this force; since the actions of each pair of particles, *n*, *n′*, will be proportional to this displacement.

Now if we take any particle *n* of the section *cd*, and consider the actions on it of two particles *m*, *m′*, at equal distances above and below *n*, and at the same distances that *n* and *n′* are above and below *m′;* then, if the actions of *n* and *n′* on *m′* are such as to give a resultant directed upward, the actions of *m* and *m′* on *n*, will give a resultant directed downward, as shown by the arrows in the figure. These two resultants will be equal to each other. It follows, therefore, that the entire action of *ab* on *n* will be represented by a force acting downward equal to that by which *m′* is drawn upward by the action of *cd* upon it. This force will then be equal in intensity to the extraneous force. Accordingly the extraneous force will be transmitted from *ab* to *cd;* and in the same manner from this section to the next, and so on to the point of support. The transmission is effected by the slipping of each vertical section, or lamina, by the same amount upon the next, and so developing reciprocal vertical actions equal to the extraneous force.

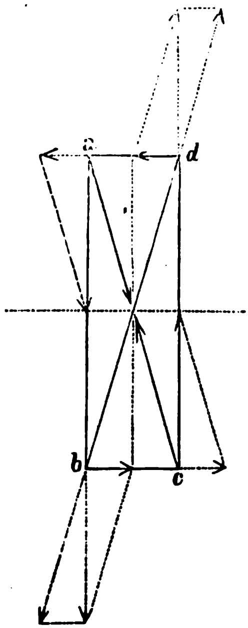
Let us next seek to determine the longitudinal strains on the fibres, developed by the extraneous force in the process of lateral transmission just considered. Let *ab* and *cd*, Fig. 2, represent two vertical cross sections of the lever indefinitely near to each other, of which *ab* directly receives the force applied to one end of the lever. The relative slipping of contiguous laminæ causes the rectangle *abcd* to take the figure of an oblique parallelogram, $a'b'c'd'$; the diagonal ac being shortened, and the diagonal bd being lengthened. It therefore developes forces of compression taking effect from a' and c' toward the centre o of the parallelogram, and forces of extension taking effect from o toward b' and d'. The reactions to these forces take effect from o toward a' and c', and from b' and d' toward o. These reactions will urge the points a' and d' toward the left, and the points b' and c', toward the right; and the longitudinal strains on the fibres $a'd'$ and $b'c'$, thus originating at the angular points of the parallelogram, will be equal. For, supposing that there are only these extreme fibres, and that the diagonals $a'c'$ and $b'd'$ are material lines, the figure $a'b'c'd'$ is to be regarded as a system in equilibrium under the action of two equal forces along its vertical sides; that along $a'b'$ being the active force and directed downward, and that along $c'd'$ being the equal reaction of the fixed support transmitted to $c'd'$, and directed upward. One-half of each of these vertical forces will act at the upper and lower corners of the parallelogram. The reactions along the diagonals, above alluded to, will at these points sustain the equal vertical forces acting on them, and at the same time develope equal longitudinal strains on the fibres $a'd'$ and $b'c'$. Or, more directly we may regard the equal vertical forces, at the four angular points, as taking effect at the same time along the diagonals and along the fibres. This is illustrated in Fig. 3, in which the vertical forces soliciting the angular points of the parallelogram are represented by the halves of its vertical sides, or by equal lines. It will be seen that the longitudinal strains developed at these points will be represented by the halves of the horizontal sides; and therefore that the entire strains on the extreme fibres, due to the parallelogram considered, will be represented by these sides, ad and bc. If now we take the case as it actually is, and regard the entire area,

Fig. 2.



abcd, as made up of fibres, the only result will be that the longitudinal strains which, upon the previous supposition, would be developed along *ad* and *bc*, will be distributed over all the fibres lying between these extreme fibres and the middle one. What the law of the distribution may be it does not concern us now to inquire. It is plain that the individual strains will decrease from the outer fibres toward the middle one, where there will be no strain.

Fig. 3.



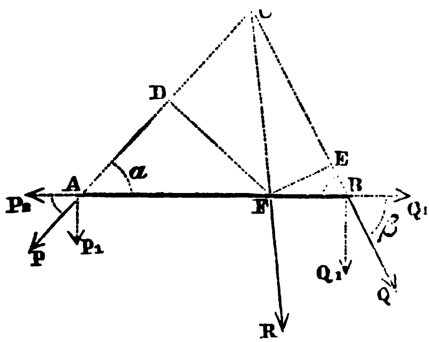
If now we take another vertical section indefinitely near to *cd*, it will form with *cd* another parallelogram, the vertical sides of which will be solicited, in opposite directions, by the same forces as those of the parallelogram just considered. The same strains as before will therefore be developed along the upper and lower fibres by these forces. The same will be true of each successive parallelogram into which the arm of the lever may be divided. The actual strain along any one continuous fibre, at the fulcrum, will therefore be equal to the strain on this fibre developed by any one of the parallelograms, multiplied by the number of parallelograms in the extent of the arm of the lever. Now if we suppose the two lever arms to be of unequal length, whatever may be the comparative intensities of the two forces that balance each other, each will give rise to a slipping of contiguous vertical sections, or laminæ, of its lever arm, proportional to those intensities, and so develop longitudinal strains on any fibre, in the extent of any single parallelogram considered, proportional to the same. Let p and q represent these proportional strains on a single fibre, and P and Q the forces applied to the lever, then $p : q :: P : Q$. Let m represent the arm of lever of P, and n that of Q. The number of equal parallelograms contained in these lever arms will be proportional to their lengths, m and n. The strain on the fibre considered, at the fulcrum, resulting from the action of P, will then be denoted by pm, and that resulting from the action of Q by qn. But the equilibrium requires that these directly opposite strains should be equal; and therefore $pm = qn$. Hence $p : q :: n : m$; and therefore $P : Q :: n : m$.

Since each of the forces P, Q, is transmitted to the fulcrum, by the slipping of each vertical section of the lever on the next, without change of intensity, the pressure there will be equal to the sum of P and Q.

The theory of the lever of the second order, as well as of the third, is essentially included in that of the lever of the first order; since the reaction of the fulcrum of the latter may be replaced by an active force, and either of the forces P, Q, by the reaction of a fulcrum.

Case II. *Straight Lever with oblique forces.* Let $A\,B$, Fig 4, represent the lever, and P, Q, forces obliquely inclined to it,—the system being in equilibrium about some fixed point intermediate between A and B. Produce P and Q to their point of intersection C, and let $C\,F$ be the direction of their resultant R, supposing them, for the moment, to act at C. Wherever

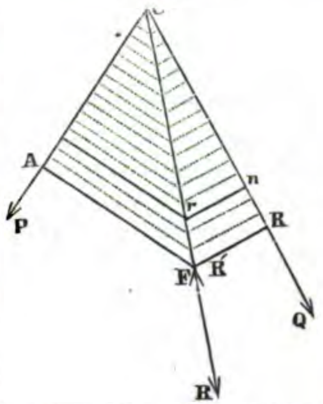
Fig. 4.



the fulcrum may have to be in order that P may balance Q, P and Q will be transmitted to it, by the process of molecular lateral transmission that has been explained, without change of direction or intensity, and therefore give a resultant pressure, R', on it, having the same intensity and direction as R acting at C. Decompose P and Q as shown by the arrows, and suppose R' to be similarly decomposed into the components R_1 perpendicular to the lever, and R_2 lying in it. Then, since R' is the resultant of P and Q transmitted to the fulcrum, we have $Q_2 - P_2 = R_2$, and $P_1 + Q_1 = R_1$. Thus P_2 and

Q_2 are neutralized by the component of the reaction of the fulcrum (which is equal and opposite to R') in the direction of the lever; since this component is equal and opposite to R_2. It follows, therefore, that the two components, P_1 and Q_1, perpendicular to the lever, will balance each other about the fulcrum. It now remains to be seen where the fulcrum must be situated, in order that P_1 may balance Q_1. From F, where the line of direction of R, through C, cuts AB, draw FD and FE, perpendicular, respectively, to P and Q, then by the parallelogram of forces, $P : Q :: FE : FD$; — and therefore P and Q may be represented by FE and FD. Now $P_1 = \mathrm{P} \sin \alpha$, and $Q_1 = Q \sin \beta$; and $\sin \alpha = \frac{FD}{FA}$, and $\sin \beta = \frac{FE}{FB}$. Hence $P_1 = P \frac{FD}{FA}$, and $Q_1 = Q \frac{FE}{FB}$; or $P_1 = FE \frac{FD}{FA}$, and $Q_1 = FD \frac{FE}{FB}$. Therefore $P_1 : Q_1 :: FE \frac{FD}{FA} : FD \frac{FE}{FB} :: \frac{1}{FA} : \frac{1}{FB} :: FB : FA$. The fulcrum is therefore at the point F, where the line of direction of the resultant R, of the forces P and Q supposed to act at C, cuts the lever. But we have already seen that $P : Q :: FE : FD$. Hence these forces are inversely proportional to their technical lever arms, FE and FD. As P and Q are transmitted to the fulcrum without change of direction or intensity, the pressure on the fulcrum will be equal to R.

Fig. 5.



Case iii. *The Bent Lever.* Let P and Q, Fig. 5, be two forces acting on two points A and B of a body of indefinite extent, capable of turning about a fixed point F, situated in the plane of the lines of direction of P and Q. Produce these lines of direction to their point of intersection C; and from F draw the perpendiculars FA and FB. Divide FC into an indefinitely great number of equal parts, and from the points of division draw parallels to FA and FB. Let us first suppose that the point C falls within the body, and that this is of uniform thickness in a direction perpendicular to $A\,C\,B\,F$, and has the form $A\,C\,B\,F$. The perpendiculars to P and Q, from the points of division of $F\,C$, will divide the body, conceived to be represented by the area $A\,C\,B\,F$, into an indefinite number of similar portions of the bent lever form, with the points of intersection of the two arms on the line $C\,F$. Now let a represent the cross section of any one of these, on the line of P, and b the cross section of any one on the line of Q. P will be equally distributed over the entire cross section $A\,C$, and Q in the same manner over $B\,C$. Let p and q denote the fractional portions of P and Q that take effect at the ends of any one of the bent lever portions of $A\,C\,B\,F$. If we consider any one of these portions by itself, it will be solicited by the forces p and q, and be in equilibrium under the operation of these forces and some portion of the reaction R', to the pressure R, on the fulcrum, produced by P and Q. They will be transmitted inward by the slipping of contiguous sections, and neutralized by a certain portion of R' transmitted along its line of direction; and the point in which this line intersects the bent lever portion under consideration must be its virtual fulcrum. Now if we consider the bent portion next to C, its fulcrum must be indefinitely near to C; while that of the first elementary bent lever, $A\,F\,B$, lies at F. But all the fulcra of the elementary levers must lie on the line of direction of R', which passes through F. Hence, as C is the fulcrum of one of these levers, this line of direction must also pass through C. Now the direction of R' is opposite to that of the resultant R, of P and Q supposed to act at C; since R' neutralizes P and Q transmitted, without change of direction or intensity, to F. Therefore, as its line of direction passes through C, it must coincide with the line of direction of the resultant R, of P and Q supposed to take effect at C. It follows, then, that the fulcrum, F, lies on this resultant. But, by the principle of parallelogram of forces, we have, for any point, F on the resultant, the proportion $P : Q :: F\,B : F\,A$.

Now suppose a portion, $C\,m\,r\,n$, of the body $A\,C\,B\,F$, to be removed; the equilibrium will not be disturbed, since the only effect will be to augment the intensities of the fractional parts of P and Q that act on the elementary bent levers of the remaining portion, $m\,r\,n\;B\,F\,A$, without altering their ratio. Hence the forces, P, Q, which act on a bent lever and hold each other in equilibrio, are inversely proportional to their lever arms.

It will be seen from what has been stated, *in what manner the forces P and Q become neutralized by the action of R', or the reaction of the fulcrum.* Even when their point of intersection, C, falls within the body, they cannot be regarded as actually transmitted to C, and taking effect wholly there in opposition to R' transmitted and taking effect wholly at the same point. As a matter of fact it is only an infinitesimal portion of each force that operates at C. An equal indefinitely small portion, p, q, or r, acts upon each elementary lever; and each triplet of forces taking effect on each elementary lever, counteract each other. In other words, the forces P and Q, instead of taking effect by direct transmission, wholly either at C or F, are actually distributed along the whole length of the portion of the line $F\,C$ that falls within the lever, and are there neutralized in equal elementary portions, by the corresponding elementary portions of R', transmitted to the same points.

If we suppose the body on which P and Q act, to have an indefinite extent, the only portion whose molecular forces will be called into play, in the transmission and counteraction of the forces, will be that comprehended within the lines of direction of P and Q. This will comprise the part $A\,C\,B\,F$ already considered, and another part lying on the other side of $A\,F\,B$, that is on the side toward which P and Q solicit the points A and B. This part may be subdivided into elementary bent levers like the other, and its existence will have no other effect than to diminish the absolute values of p, q, and r, the triplet of forces answering to any one of the whole number of elementary levers, into which the entire portion of the body that lies within the lines of direction of P and Q is divided. It will be observed that for each elementary lever of this part, the resultant of the forces p and q will take effect upon the fixed point F, as a force of traction, instead of as a force of pressure, as in the case represented in Fig. 5.

We thus arrive at the general principle, that if two forces, whose lines of direction when produced intersect each other, are applied to a body capable of turning about a fixed point in the plane of these lines, these forces will be in equilibrio, provided *the statical moment of the one force is equal to that of the other.*

III. ASTRONOMY.

1. Spectrum Observations at Burlington, Iowa, during the Eclipse of August 7, 1869. By C. A. Young, of Hanover, N. H.

The instrument employed was the same referred to in my next paper "on the use of the spectroscope in observing contacts at a solar Eclipse." A comet seeker of four inches aperture and thirty focal length, throws, by the help of an eye-piece, an image of the sun, about two inches diameter, upon the slit of the spectroscope. The collimator and observing telescope of the spectroscope, made by Clark, have each an aperture of two and a quarter inches and a focal length of about seventeen inches. The dispersion is produced by a train of five heavy flint glass prisms with angles of 45°, each, and faces two and a quarter by three inches. They give a refraction of 165° (for the *D* line) and a dispersion of about 18° from *A* to *H*. The eye telescope is carried over the spectrum by a tangent screw, but all determinations of the positions of lines have to be made by a micrometer in the eye-piece. The instrument is provided with an arrangement for throwing an air spectrum formed between platinum wires by the spark from an induction coil, into the field of view along side of the spectrum under observation.

While the eclipse was coming on, special attention was directed to the moon's limb, in order to detect if possible indications of an atmosphere. The results were solely and remarkably negative. The dark lines of the solar spectrum came up squarely and exactly to the moon's limb without the slightest curvature or distortion, whether the slit was tangent or perpendicular to it. The contrast in appearance between the limb of the sun and that of the moon was very striking. When the image of the sun's limb was brought to the middle of the slit the line of demarkation between the bright and dark portions of the spectrum was always more or less hazy, however carefully the instrument might be put in focus; different portions of the limb also differed very much from each other in this respect, some being far less sharp than others; but when the moon's limb was similarly placed it was as sharp as could have been drawn with a pen. Of course the cloud-like nature of the solar photosphere accounts for its comparatively indefinite outline.* Secchi's *continuous* spectrum at the edge of the sun I have never seen; the stratum which produces it (if there is such a stratum) must have a thickness little if at all exceeding 1″. During the totality there was in the field of the spectroscope a faint continuous spectrum, undoubtedly from the corona, without any visible traces of *dark* lines; a fact rather surprising, considering the polarization of this light, which was so complete that when the axis of a tourmaline, held in the hand over the eye-piece, was placed parallel to the limb of the sun the faint spectrum nearly vanished.

Upon this spectrum were superposed a number of bright lines when the image of a prominence was thrown upon the slit. Nine of them were seen by me, viz.: *C*, *K*, 1017.5 (near *D*) and which for convenience I will refer to as the D_3 line; a faint line near 1250 *K* (by estimation), another faint line about *K* 1350 (also by estimation), a conspicuous line *K* 1474 (3503 of my scale) just below *E*, *F*; a pretty conspicuous line at *K* 2605 $\pm$ 2 (by measures), *K* 2796, just below *G*, and *h*. I did not see any line at *b*, but presume it was passed over without proper examination in the manner I will presently explain. There can be no doubt I think, that it was conspicuous enough in the great prominence on the *W* edge of the sun, as seen by others. But in sweeping along the spectrum with my tangent screw beginning at *C*, I noticed on coming to the 1474 line that it extended *clear across the spectrum* which was not the case with the *C* and *D* lines, since the protuberance on

* This polarization may have been produced, as suggested by Prof. Pickering, by the successive refractions at the faces of the prisms.

the *SE* limb then on the slit only had a height of about 1½′. (The slit is 2′ 54″ in length.) This fact at once arrested attention and led to the suspicion that this line, *K*, 1474 (which I first noticed in July, and described in the last number of the Journal of the "Franklin Institute," published a few days before the eclipse) was not a protuberance line at all, but *due to the corona;* and on moving the slit of the instrument entirely off from the protuberance this line remained, while the golden D_3 line disappeared.

It now became necessary, on account of the moon's advance, to bring the other side of the sun's image to the slit by the right-ascension screw; and I imagine that while the assistant was doing this I continued to carry along the spectroscope-telescope by its tangent screw, thus passing over the region of *b*, while the telescope was pointed nowhere in particular. But I am ashamed to say that I have no recollection at all about it, nor can I recall distinctly the behavior of the two faint lines between *D* and *E*, though my strong impression, as recorded immediately after the eclipse, was that they also were, like 1474, *corona* lines. The line *K*, 2602, had its position determined by micrometrical reference to *K*, 2796, and just as that measurement was completed the sun came out, and all the new lines, not previously observed in full sunlight, instantly disappeared. The feeling of chagrin and surprise at thus being cut short was almost overpowering—the sense of wasted opportunity and personal imbecility. No line was seen at *K*, 660, below *C*, where I have twice before seen and measured one. I saw nothing of the difference between the spectra of the different prominences reported by some of the observers, but as I observed one part of the spectrum with one prominence in the slit, and the rest with another, it need not be wondered at. While the great dispersive power of my instrument gave great advantage for the observation of faint lines, and the accurate determination of position; it had also great disadvantages in the smallness of the field, and the time required for an exploration of the whole spectrum.

But I wish to call the special attention of the section to another point in this connection. The line *K*, 1474, in the *corona spectrum* coincides with a line in the spectrum of the

Aurora Borealis reported, by Prof. Winlock, in the last number of "Silliman's Journal," at 1550 of Huggins' scale, as nearly as I can determine by the comparisons of the two scales given in Prof. Gibbs' recent paper on wave lengths by the method of comparison. If the lines do not absolutely coincide, the distance between them can hardly equal one division of either scale. The first Aurora Borealis will enable any one, with a spectroscope, to settle the matter accurately by measuring the distance from *b* (produced by burning a bit of magnesium before the slit) to this line. I have been very much disappointed at not having had an opportunity to do this before reading this paper. Furthermore, two other of the five Aurora lines reported by Prof. Winlock, viz.: 1280 and 1400 of Huggins' scale agree, fully within the limits of error, with the two faint lines between *D* and *E*, which I suppose are also corona lines. Taking these facts in connection with the peculiar radiating structure of the corona and some of the prominences, and their rapid changes of form and brilliancy, does it not become very probable that our own Aurora Borealis, and these wonderful emanations from the sun are kindred, if not identical pheonomena?

Possibly, hereafter, spectroscopic observation of the prominences, and of the intensity of this corona line, which though rather difficult in my instrument could be easily observed with one of higher dispersive power, may lead to farther light when combined with magnetic observations.

This line is given by both Kirchoff and Angstrom as an iron line, though faint and very close to two other much more conspicuous lines produced by the same substance; the same is also probably true in respect to the two other lines of the corona, which appear to coincide with lines given as iron on the charts. It is certainly worth inquiry if there is not some misconception in this, and whether, since *iron* can scarcely be conceived to exist in a gaseous state in those terrestrial regions where the Aurora Borealis resides, it may not always have associated with it some other substance, which also can exist separately as a gas of inconceivable tenuity. But this belongs to the chemists.

I ought to add here, that since the preparation of this paper

Prof. Winlock has informed me that he has thought of, and published, the same idea as to the probable identity of our Aurora and the Corona, though not based on any comparison of spectra.

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2. On a New Method of Observing the First Contact of the Moon with the Sun's Limb at a Solar Eclipse by means of the Spectroscope. By C. A. Young, of Hanover, N. H.

At the recent solar eclipse, observed by the writer at Burlington, Iowa, as a member of the party, under the direction of Prof. Coffin, Superintendent of the American Nautical Almanac, the instant of the first contact was determined by means of the spectroscope, in a manner which is believed to be entirely novel and to admit of much greater accuracy than has ever been attainable heretofore in this kind of observation.

In the instrument employed, a telescope of four inches aperture and about thirty inches focal length (a so-called comet-seeker) was arranged to throw an image of the sun about two inches in diameter upon the slit of the spectroscope. The slit is one-eighth of an inch long; its width adjustable. A circular plate of brass about two and a half inches in diameter, with a hole also one-eighth of an inch in diameter at its centre, was attached by a hinge in such a manner that it could be turned down over the slit; it was covered with paper and graduated by radiating lines into angles of 10°, and furnished the means of bringing on the slit the portion of the sun's limb which was indicated by calculation as the point of contact.

A train of five flint glass prisms, with refracting angles of 45° formed the spectrum which was viewed by a telescope of two and a quarter inches aperture (the same as the collimator) and about seventeen focal length, with a magnifying power of eighteen. The spectrum, referred to the limit of distinct vision, was about one and three-fourth inches wide by four feet long. The whole arrangement was mounted equatorially, with slow motion screws, etc. I have been somewhat particular in describing the instrument as it is likely the method about to be presented for your consideration might prove impracticable with a single prism instrument. High dispersive power is neccessary. The equatorial mounting is also important.

When now the image of the sun's limb is made to bisect the slit at right angles or nearly so, the spectrum will present something the appearance indicated in the diagram. One-half (formed by the sun) will be brilliant, the other will be comparatively dark, being formed mostly by the light reflected from the air in the sun's immediate neighborhood. The spectrum of the sun's chromosphere, however, is superposed upon the air spectrum and becomes visible in certain bright lines, especially the *C* line, which is by far the most easily observable; and the effect is this: most of the dark lines extend clear across both portions of the spectrum, the dusky as well as the brilliant; not so with the *C* line. This is intensely black on the brilliant portion of the spectrum, but is continued in the dusky portion by a little needle of brilliant red light, of a length ranging in my instrument from an eighth of an inch upwards. When a protuberance is in the slit it often reaches clear across, but generally is not more than a quarter of an inch long. In other words, the *C* line instead of simply extending across the whole spectrum like the other lines, has a portion of its length, beginning at the boundary between the bright and dusky portions and extending into the dusky portions, replaced by a *bright* line. This bright line is usually somewhat *pointed* at the outer end.

Now suppose that the portion of the limb on the slit is that where the moon is to strike. As the depth of the chromosphere is seldom less than ten to fifteen seconds of arc, the moon will reach *its* outer edge some twenty or thirty seconds before the time of true contact with the sun's limb, and the observer will see the little *red needle* which I have described; first squarely truncated,—its point cut off—and then gradually and steadily shortened until at the instant of actual contact it disappears, and the *C* line becomes exactly like its neighbors. The observation in this way is as easy and certain as that of the transit of a moderately slow star. I do not think an error of more than half a second possible with reasonable care, and it ought to be less.

At the eclipse of August 7, I saw in the manner I have described, the moon's approach to the sun's limb, and noted its contact about five seconds earlier than any of those who were observing by the ordinary methods, and differed among themselves some ten seconds. I have been much gratified also to learn from Prof. Mayer, who had charge of the photographic operations of our party, that the time of first contact, deduced by measurement and computation from a photograph taken as soon as possible after the contact had been signalled to him from the other observers, agrees with my own observation within three-tenths of a second.

The same method would of course apply with slight modifications, to the observation of the other contacts, but I am not aware that it presents any advantage over the older methods.

I desire to call special attention to the applicability of this method in the approaching Transits of Venus. Undoubtedly the atmosphere of the planet, and irradiation will interfere with its accuracy to some extent, but I think no more than with other methods, and it must certainly be much to the observer's advantage to be able thus to *perceive*, if he cannot be properly said to *see*, the planet before she strikes the sun, and watch her approach.

At the internal contact also, I think the sudden formation of a long horizontal line of light extending the whole length of the spectrum would be a much better phenomenon to observe accurately than the rupture of the black ligament, about which so much has been said in this connection.

I cannot help feeling that this method of observation ought to be provided for in any expeditions that may be sent out to observe the Transits.*

*Having lately received the volume of the *Comptes Rendus* of the French Academy of Sciences for the first half of the current year, I find that I have been anticipated by M. Faye in respect to this method of observing the contact of an opaque body with the disc of the sun. At the session of Monday, January 11th, 1869, in a discussion upon the observation of the coming transit of Venus, he proposed essentially the same use of the spectroscope which I have described in the foregoing article.

3. Observations on the Eclipse, with an Arago's Polariscope at Mt. Pleasant, Iowa. By Edward C. Pickering, of Boston, Mass.

Observations of previous eclipses have led to the belief that the light of the corona is polarized in planes passing through the centre of the sun, and therefore that it shone by reflected light. It was with the expectation of verifying these views that I prepared an Arago's polariscope to observe the recent eclipse. This instrument consists of a tube closed at one end by a double image prism of Iceland spar, and at the other by a plate of quartz. Looking through the spar we see two images of the quartz of complementary colors (in this case blue and orange) when the light is polarized. Although this polariscope is somewhat less delicate than that of Savart, yet it enables us to determine the condition of the light emanating from every part of an object, by noting the color of its image. If, now, the corona was polarized as above described, in one image the upper and lower parts would be blue, those on the right and left yellow; while in the other image these colors would be reversed, the yellow above and below, and the blue on the sides. In reality the two images were precisely alike, and *colorless*. But one was on a *blue*, the other on a *yellow* background. Consequently the corona was not polarized. This does not prove that the corona is self-luminous, as the light reflected by the clouds is unpolarized, and in general polarization is produced by specular, and not by diffuse reflection. The colored background proves that the sky close to the corona was strongly polarized in a plane which was independent of the position of the sun, since it was the same on all sides of it.

The following explanation of this curious effect is suggested. The earth beyond the limits of the shadow is strongly illuminated and acts as an independent source of light. The reflection of its light by the sky, would produce the effect just described.

The corona was also examined by a double image prism of spar, and by this, also, was seen to be unpolarized.

IV. ELECTRICITY.

1. Causes of the Failure of Lightning Rods. By James Bushee, of Worcester, Mass.

Perhaps few subjects can be deemed of greater practical interest to the public than that of protecting life and property from the effects of atmospheric electricity.

Since the time of Franklin, the well known metallic rods have been used in this and other countries, as a means of security, and scientific men have very generally, if not universally, acknowledged their utility, when the laws of electricity are properly regarded.

In this section of New England,* thunder storms are common during the summer, and sometimes very violent, while certain localities seem to be more especially exposed to frequent and powerful electric discharges.

In some casual observations of the effects of lightning, my attention was called to the significant fact that a large majority of the buildings injured by lightning had rods attached to them for the purpose of protection.

Instances were not unfrequent where dwelling houses, with rods, were "*struck*" and damaged, while those in the immediate vicinity, without rods, escaped unharmed ; sometimes houses with no rods have stood many years uninjured, but soon after the application of rods they have been struck by lightning.

Such facts have led some candid and intelligent persons to doubt the efficiency of lightning conductors, while others have been induced to reject them altogether as wrong in principle and dangerous in practice. But error in the interpretation of phenomana, or in the application of a scientific truth, does not invalidate the integrity of the principle itself. The fundamental idea of Franklin is doubtless correct, and the unfavorable results so often witnessed are but the legitimate fruits of wrong application. If so it is evident the cause of failure lies in defects which have a potential remedy, and that too in the truthfulness of the very law which is often unjustly made

* Worcester, Mass.

responsible for consequences—for the evils invariably disappear just in proportion as we are enabled to fulfil the highest conditions of that law.

What then is the cause of the many casualties from lightning where rods are used?

With the view of meeting this enquiry I have made it a special point for several years past to examine, as carefully as I could, the principal accidents which have come to my knowledge in Worcester and vicinity; also various cases of interest in other localities.

From obvious results of observation it appears:—1. That buildings have been struck in numerous instances, and more or less damaged, with all the different kinds of rods commonly used in New England, irrespective of form, style or material—whether of iron or copper—not, however, that all are equally defective, or that all are constructed with equal fidelity and scientific skill, but none have completely fulfilled the object intended. 2. That the leading defects of rods in general, and the cause of failure to protect buildings, are due, in part, to their construction and arrangement, but *principally to their imperfect connection with the ground, and the inadequate conducting power of the materials with which the rod comes in contact.*

As regards the construction and application of the rod the directions usually given by good writers on the subject are all, perhaps, that could be desired, if they were properly observed by constructors; but first principles are too often sadly overlooked in the outset—besides the rod is liable to become defective by long neglect. The joints should be kept in perfect contact to preserve the entire continuity of the conductor. If the rod is badly rusted, especially the part connected with the ground, its capacity for conveying a heavy charge is materially impaired. When the tip of the rod becomes oxidated and blunt the charge is liable to become more sudden and disruptive, whereas a sharp and perfect point receives the electricity *gradually* and begins to effect a discharge at a greater distance, giving time for the whole charge to be harmlessly conveyed to the earth.

Short crooks towards the building, and abrupt changes in the size or conductivity of the rod, tend to induce lateral discharges. The rod should not diminish in size or capacity from the top downwards, but rather increase in conducting power as the charge descends.

An objectionable feature in the application of the rod appears where two or more *receiving* points are erected upon the roof, and terminated in a single rod extending to the ground, or where the rod is turned upward from the point, over some higher parts of the roof, even higher than the point itself.

In such cases it is found that the lateral discharge usually takes place at the lowest point of the bend, or at the point of junction where two or more rods terminate in one.

Independent of any hypothesis, as to the nature of electricity or its mode of action; whether any distinct fluid ever passes up or down the rod in case of a thunder storm; or whether all the effects witnessed are due simply to induction and polarization, there is no less actuality and power in its operation. The agency with which we have to deal is no less real or tangible in its effects. We may therefore regard electrical phenomena as the result of a dynamic force which bears a striking analogy, in some respects, to other forces that are governed by well known dynamic laws. So far then, as this analogy holds, it may lend some aid in forming correct conclusions.

Suppose for example a rod has two arms, or branches, erected perhaps upon the chimneys of a house, and forming a connection with the main rod somewhere on the roof; and suppose, also, that the electric force is conceived to act from the top downwards, requiring time for its transmission through the rod. Now should a heavy charge be received at the same time upon both arms of the rod, the two electric forces pursuing the irrespective channels of conduction, rush together at the point of junction, where, for an instant, the intensity is greatly increased, while the single rod below this point being no larger than either branch, and having but half the conducting power of both, is unable to carry the double charge so suddenly imposed upon it, and hence a portion of the burden is forced to seek an unnatural channel of escape through the building.

A moderate charge even, which the capacity of the rod might easily convey, under favorable circumstances, may became so intensified at certain points, by a sudden augmentation of electricity—as by the meeting of opposite currents, or an abrupt reduction of conductivity—that a lateral discharge would ensue before requisite time could elapse for the electric force to adjust itself to the new condition of things. This arrangement appears to be equivalent to reducing at once the conducting power of the rod to half its former dimensions.*

The second and principal cause of failure is due to the defective mode of connecting the rod with the ground; the inadequate means provided for the escape of the charge from the conductor into the earth.

It is a common practice to run the rod into the ground from four to six feet, or until it is supposed to reach the moist earth, and this is generally considered quite sufficient for all practical purposes, and the numerous agents employed by different manufacturing companies to supply rods throughout the country, seem to believe that when thus much is done their duty is fully accomplished, and that the rod can then be left, in good faith, as a safe and reliable protection.

It is well known that loam, sand and gravel, are comparatively poor conductors of electricity, and experience proves that these materials, in the ordinary modes of fitting the rod, furnish very inadequate means of conveying a heavy charge to the earth.

There is generally no enlargement or extension of surface where the conductor enters the ground, but merely a continuation of the same rod used on the building, thus presenting a very limited surface to partial contact with imperfect conducting materials. It is true a light charge under such circumstance may be safely carried away, but when the cloud furnishes a copious supply of electricity and the point is in a perfect condition, the rod receives the charge with great facility, and if its passage to the earth is opposed by inferior conductors, a disruptive discharge through some part of the building would naturally follow.

*The defects of this arrangement, so far as the above remarks are designed to be more especially applicable, appear more prominent when other defects exist; when the tip of the rod is imperfect, and the charge enters upon it more or less abruptly, or when the resistance at the earth prevents a free escape of the charge. If the rod were in a perfect condition, in other respects, accidents from this construction would perhaps rarely if ever occur.

If the rod were perfectly insulated it would receive the electricity as before, but in this case the whole charge would leave the rod by an explosive discharge. Again were the rod partially insulated, that is connected with the earth by poor conductors, then a part of the charge would be gradually and silently transmitted through these conductors while the surplus would escape by explosion.

The relative power of the point to receive, and of the material connected with the rod to impart electricity, is a practical question in its bearing on this subject, and worthy of more careful attention than it has generally received.

Where the resistance is very considerable, as it always must be when the rod is merely inserted in the ground as usual, the points receive electricity from the clouds with greater freedom than it is discharged to the earth, and the liability of accident increases in proportion as the receiving power of the rod exceeds the discharging power. A way is opened through the pointed conductor for a free passage of electricity from the cloud to the rod, but the channel of escape to the earth is practically closed.

The rod thus becomes, so to speak, a reservoir of intense electric force, and when the tension is increased beyond certain limits the surplus charge, yielding unwilling obedience to forced circumstances, breaks away from its proper channel.*

In such cases the tip of the rod is often burned or fused, the glass insulators, if such are used, broken and thrown from their places, and frequently the ground where the rod enters more or less disturbed and torn up.

The point of lateral discharge may be determined by various circumstances;—by a short bend in the rod towards the building; by the junction of two or more branches of the rod; by the near approach of a good conductor, as a bell wire, or gas pipe, etc.; stove pipes, iron ware, etc., stowed away near where the rod passes.

These remote influences, however, must not be mistaken for the primary cause of the casuality. In case of the explosion

*This condition of the rod may be conceived, as somewhat analogous to the hose of an hydraulic engine, with a forcing pump plying at one end while the other end, instead of admitting a free outlet for the water, is firmly closed except some lateral apertures.

of a steam boiler, under extreme pressure, the slightest irregularity in the uniform thickness or strength of the boiler would be sufficient to determine the point of rupture. So when a lightning conductor is subjected to all the charge it can bear, in consequence of resistance at the earth, a very slight circumstance would turn the balance of force in favor of a lateral discharge.

There is, indeed, good reason to believe that nine-tenths of the cases noticed, where the rod has failed to accomplish its purpose, are really due to *an overcharge, caused by the resistance of the passage of electricity into the earth.*

In the most severe disasters the rod is invariably found to be very imperfectly connected with the ground, being usually much oxidated, and often extending not more than two or three feet into dry earth, or into sand or gravel nearly dry, by which its conducting communication with the ground is almost entirely destroyed.

But the general effects are observed to be less and less marked, as the condition of the rod is improved and resistance to a free exit of the charge diminished.

There are three kinds of rods very common in New England, viz.: Lyon's Patent Copper Rod, the North American Lightning Rod—galvanized iron, and the Quimby Rod, made of small nail rods in their rough state.

Perhaps a less proportion of serious accidents have occurred with Lyon's rod than with either of the others mentioned, yet more than eighty cases of this rod have been reported from Worcester county alone, where slight damage occurred, or more or less evidence appeared that a charge had passed the rod.

Sometimes the scorching or burning of leaves and grass near the rod would be the only traces of electricity visible. In other cases the ground about the rod would be more or less disturbed and torn up, or the house perhaps slightly injured by a charge passing on to the sill, tearing off some clapboards, splitting the timber, and finally disappearing in the cellar or making its escape outside through a pool of water left by the recent shower.

Such phenomena are of very frequent occurrence, and clearly point to the same ultimate cause of disaster—the ground resistance.

In these cases the rod seemed to perform its functions so far as it could. The charge was conveyed from the clouds *to* the earth but was arrested in its progress *into* the earth, and in forcing a passage to better conductors produced the effects observed.

The tendency of a charge to leave the rod is greater at or near the point of greatest resistence, and the various effects of an overcharge, so very generally exhibited at the lower portion of the rod, sufficiently indicate the primary source of failure. While it is quite unnecessary to detail individual cases for the sake of farther illustration, a single instance of remarkable mechanical effect upon the rod may be briefly stated.

Lyon's patent consists of a copper ribbon so folded or crimped that the cross section forms an *S*. The rod is then twisted once in twelve or fifteen inches in order to give it greater firmness.

In the summer of 1859 this rod was placed upon a new house in North Brookfield, Mass., and was terminated in the embankment formed by sand and gravel thrown out of the cellar, but was not carried deep enough to reach the original soil. During the summer this gravel, so far as the rod penetrated, became very dry, and in this condition the rod was subjected to a severe charge of electricity. The consequence was the twist was completely taken out of the rod throughout its entire length, and the folds or flanges brought firmly together as if by the force of a hammer or the pressure of heavy rollers. The charge escaped where the rod entered the ground, throwing up large quantities of dry earth and making a deep furrow some fifteen or twenty feet in length, as if a large plow had passed through the ground. The house remained uninjured, no part of the charge having left the rod till it reached the earth. This indeed is an extreme case, and so far as I know unparalleled in some of its effect, yet it is but a more forcible illustration of the point in question—*the ground resistance.* Here is the main idea designed to be brought out in this part of my paper, and the prominent cause, no doubt, of the numerous casualties so dishonorable to the confidence and good faith which lightning rods should justly merit.

It now remains to consider the best means of removing the "cause of the failure of lightning rods." This will form the subject of another paper.

2. Conditions of a Perfect Lightning Rod. By James Bushee, of Worcester, Mass.

While considering the defects of a lightning rod in a former paper, the necessary conditions of a perfect security have been in some degree anticipated.

A rod performs its functions when it conveys the heaviest charge from the cloud to the earth without sensible effects; but in order to remove all liability of accident it should be capable of doing more than this.

No machine can be worked to its utmost capacity without hazard. The rod should always have some working capacity in reserve. The whole charge ought to pass to the earth with such perfect freedom as to avoid high intensity in any part of the rod, thus rendering a lateral discharge impossible, or extremely improbable.

If the earth were a perfect conductor of electricity nothing more would be necessary to secure the requisite conditions than simply to connect the rod with the ground. But since there is a wide contrast between the conducting power of the earth and that of the rod, and since this affords sufficient cause, as proved by observation and experiment, to obstruct and divert the charge, or a portion of the charge, from its proper channel, it follows that the elements of a perfect rod can only be secured by removing all obstructions from the direct pathway of the charge.

While absolute perfection is doubtless, from the nature of the case, impossible, a near approximation is easily attainable, simply by taking advantage of the extensive ranges of metallic gas pipes and water pipes, with which our cities and large towns are supplied. These, no doubt, afford the best means at command, of forming a perfect connection with the ground, and of placing the rod in a condition to perform its proper functions. And this can be done without the least possible injury to the pipes or danger to the persons connected with them. A good rod in proper contact with such pipes freely imparts to them the most powerful charge, which becomes highly diffused as it expands over an extended range of surface contact, where the electricity easily makes its escape into the earth, thus restoring equilibrium with so little resistance, that the desired object may be regarded as completely attained, so far as all practical purposes are concerned.

Since accidents, in relation to these pipes, have sometimes occurred from lightning there seems to be a strong prejudice, in the minds of some, against connecting the rods in this way. Such prejudice, however, appears to arise from not making proper distinction between a gradual and silent discharge through the medium of a pointed conductor, and a sudden or disruptive discharge attended by explosion, as when, in popular language, lightning is said to "strike" an object.

All the cases which have come to my knowledge where the pipes have been damaged or persons injured, are those of *disruptive discharge*, in which the pipes were struck when the whole charge, after passing explosively through some nonconductor, rushed upon the pipes at once, producing widely different results from anything ever observed in a gradual discharge through the agency of points.

The effects of a disruptive discharge upon the pipes may be various, according to circumstances. The joints, if connected by an inferior conductor, as lead for instance, may be injured in consequence of sudden resistance,* while a portion of the charge, perhaps, not having time sufficient for dispersion and escape, flashes up the gas burners in the immediate vicinity, with sufficient force, perhaps, to give severe or even fatal shocks to persons near them.† But nothing of this kind can

* A case of this kind occurred at New Haven a few years since, and noticed by Professor Silliman at the Springfield meeting of this Association.

The joints of these pipes were made tight by sheet lead, which was melted out for a considerable distance by a shock of electricity.

† The rector of St. James Church, Bernon Smithfield, R. I., received a shock from the gas-burner, while sitting in his study, July, 1866, which caused his death a few months afterwards.

The church was near the rector's house, and the lightning rod attached to the steeple terminated in nearly dry sand or gravel. A heavy charge passed down the rod to within about ten feet of the ground, when a lateral discharge was forced through a portion of the building to the nearest gas-pipe, producing much damage to the house and the other effects above mentioned. Also several other persons in the different neighboring houses felt more or less of the shock at the same time. Had the rods of the church been connected directly with the gas-pipes I believe the charge would have been carried off with perfect safety to all, and no one would have experienced any sensible evidence that a charge had passed the rod.

possibly occur when the charge is received through a pointed conductor properly connected with the pipes.

In the former case the charge bursts from the cloud at once and dashes upon the object, as it were, with concentrated force, producing the well known effects of a "stroke of lightning."

In the latter, like a continuous stream of water from a fountain, the cloud pours out its electricity upon the rod, gradually from which it is promptly conveyed to the earth as harmlessly as it was received. When a mill-dam, confining a large pond or reservoir, suddenly breaks away the water rushes out with great violence, overflowing the banks of the stream, carrying off bridges, and sweeping away everything in its course.

This is analogous to a disruptive discharge of electricity. But when the reservoir is drawn off *gradually*, through a proper gateway, the water is confined within the banks of its own natural channel, and quietly pursues its course to the ocean. This resembles a gradual discharge of electricity by a pointed conductor, when the long ranges of metallic pipes serve as safe channels of conveyance into the earth. And we have no more real cause to apprehend danger in one case than in the other.

When such pipes are not accessible, a well, spring, or permanent stream of water affords, perhaps, the next best means of forming a connection with the ground. Water furnishes a much more perfect contact with the rod than earth and gravel, and being a much better conductor than these, it is usually regarded as a safe and reliable mode of applying the rod, to terminate it in a well or spring of water. And this idea is doubtless correct in general, but a well may be so situated on a bed of clay, or in a ledge of rock, that resistance would sometimes be sufficient to cause lateral discharge. It is to the difference between the receiving power and the discharging power of the rod that we are to look for accidents, rather than to the absolute conductivity of the rod as compared with water or other substances with which the rod may be in immediate contact.

The rod may sometimes be connected with a good conductor, which may itself be partially insulated from the general conducting medium of the earth by certain geological conditions.

That a lightning rod will receive a charge of electricity with greater facility than a well of water — under the circumstances mentioned, disposes of it — seems to be proved by the fact that a portion of the charge which had descended the rod to the water, has been known to pass up the lead pipe of the pump into the house and do serious injury.*

It would not always be safe, therefore, to terminate a lightning rod in a well which is connected by metallic pipes with the domestic departments.

A common water cistern also furnishes an unsafe mode of terminating the rod, since the brick and cement ordinarily used offers very considerable resistance to electricity. It is for this reason, no doubt, that cisterns are frequently exploded by a charge of electricity communicated through metallic water pipes connecting the eave trough with the cistern.

When neither of these modes of connection is practicable, something more is necessary to insure safety than the very common way of simply inserting the rod a few feet into the ground, which often becomes very dry during the heat of summer, the time when thunder storms are most frequent and protection the more needed.

Yet it is probably true that three-fourths, if not seven-eighths of the lightning rods in the country terminate in this manner.

When we consider the great want of fidelity, and of a thorough practical knowledge of the subject, so often manifested on the part of the agents or contractors, and the equally apparent want of care and attention on the part of those whose deepest interest is involved, it is not surprising that the record of casualties in Worcester county discloses the remarkable fact that *a large majority of the buildings damaged by lightning are those having rods attached to them.*

So little attention has generally been given to the proper connection of the rod with the ground, so little importance at-

*The editor of the Woonsocket Patriot (S. S. Foss), states, that the lightning rods of his house run into a well in the cellar, which is located on a ledge of rock, and in the summer of 1868, during a violent thunder storm, one of his domestics received a severe shock from a pump connected with the well by lead pipe. He also states that on examination he found the water low in the well, but the rod extended below the surface of the water some distance at the time of the accident.

tached to the necessary conditions upon which the efficiency of the rod depends, the free exit of the charge, that instances are not wanting where rods have been erected upon public buildings without the least apparent regard to the conducting power of the materials with which the rod was connected, and with as little thought of the means by which it was to fulfil its proper functions.*

The numerous accidents which are constantly presenting themselves, where rods are concerned, sufficiently indicate either a very mistaken idea of the necessary conditions of safety, or an unpardonable neglect in carrying out those conditions.

I have sometimes found but one small rod connecting the roof of a dwelling house with the ground, and that extending only two or three feet into nearly dry gravel.

In such a case the rod is worse than useless; for instead of protection the most effectual means are unwittingly used to invite into the house the first charge of electricity that chances to pass the rod.

The various kinds of soil with which the rod is liable to be connected are widely different in conducting capacity, and in deciding the reliability of the rod it becomes important to determine the conductivity of the ground as modified by geological character and physical condition; and this may be easily done so far as it practically affects the object of the rod by measuring the resistance the earth offers to the passage of an electric current through the rod after it is placed in position.

But probably no kind of soil, even under the most favorable conditions, possesses sufficient conducting capacity to insure safety while in the ordinary state of moisture, and simply in contact with a small rod. I repeat then some other course must be adopted; and here I have nothing better to offer than to urge the necessity of carrying out in general practice the directions usually given by good writers on the subject, but

*When one of the largest churches in this city was first furnished with lightning rods, the main rod attached to the spire was allowed, it is said, to terminate in a boulder of granite, in which a hole was drilled for the purpose. This was done with the impression, if I mistake not, that the solid rock would be more than a "*match*" for the lightning.

which are almost universally neglected in this country,* viz.: to enlarge the surface of the rod in the ground by attaching to it a plate of copper which should extend to a sufficient depth to reach permanent moisture and then be surrounded by a liberal quantity of fine charcoal which is a good conductor.

For convenience the plate may be turned out from the building horizontally to any distance it may be found necessary. No definite directions of general application can be given as to the size of the plate or its depth below the surface, since the circumstances are so very different in different localities. What would be entirely sufficient for one place would not answer for another. The capacity of the plate and connection with moisture should be such as to remove all undue resistance, and this must be determined experimentally for each particular locality. In this arrangement the deficiency of conducting power is compensated by increasing the surface contact with the ground, on the principle that a greater number of poor conductors may be equivalent in efficiency to a less number of better conductors.

But buildings may sometimes be located where circumstances of some nature render it impracticable to carry out any of the methods proposed so as to form a proper ground connection. In such cases points may be used as an efficient means of relieving the rod so as to prevent a disruptive discharge. The rod may be furnished with several short points directed from the building, and its lower extremity connected with a copper ribbon which should have all the advantage of ground connection circumstances will allow, and extend considerable distance from the building (twenty to forty feet), terminating in a short rod with points at top and also near the ground. These lower points serve the double purpose, first, of exhausting surcharged atmosphere near the surface, and second, of dissipating and weakening a charge from the clouds.

I see no reason to doubt that any portion of the charge which may not have been conveyed into the earth by conduction would be dispersed by convection without surcharging the rod. The objection may here arise, that the lower points by

* The prevalent defective mode of making the ground connections, probably obtained on the score of *economy*; but that is very poor economy which neglects the absolute conditions of safety and incurs the first expense of the rod merely to expose the house to greater danger than before.

imparting the electricity to the atmosphere do not effect a positive discharge — that the charge is merely transferred from one locality to another, even nearer the house than before, and perhaps exposing it and other buildings in the vicinity, to still greater danger.

But I think it must be conceded that such a transfer could hardly be made without, at least, a decided loss of electric force. Indeed every leaf twig and blade of grass, within the influence of the charge becomes so many little lightning rods to convey it into the earth.

Furthermore should a surplus, from any cause, tend to preponderate in the manner supposed, the very points which imparted it would be as ready to receive and convey it through the rod into the earth, or at least a portion of it, while the residual may ascend the rod and be imparted to the atmosphere above, and possibly several such transfers may take place even within a very short interval of time, yet weakening, in force, at every vibration, until compensation should be restored.

In regard to houses with metallic roofing, it is supposed by some that they are not so liable to be struck by lightning as other houses under similar circumstances — that such a roof serves, in some way, as a shield of protection against the effects of lightning, but no good reason appears for such a supposition. It is very probable, however, that a charge may sometimes be received upon the highest parts of the roof as it often is upon telegraphic wires, and be again diffused into the atmosphere through the sharp corners and edges of the lower portions of the roofing. Indeed if the usual points were erected upon such a roof and the eaves furnished with a number of good points there is little reason to doubt that they would prove a reliable protection to the building.

But since houses with such roofing have been sometimes struck by lightning and seriously damaged, one or two cases having occurred within my knowledge,* where the roof was literally destroyed; it would be well to take all possible precautions for safety. Good points should be erected on the

* In the summer of 1859 the house of Mr. Muzzy of Spencer, Mass., was struck by lightning and much injured. The house was without rods and the roof covered with tin. The charge entered the chimney through which it passed outward on to the roof, perforating and tearing up the tinning so generally that it was necessary to replace it by an entire new covering.

most prominent parts of the house, particularly upon the chimneys and well attached to the metallic surface.

In making the ground connections, however, it is sufficient to affix the rods to the corners or edges of the roof, carrying out the directions already given in their terminations with the ground. Whenever proper contact cannot be made so as to remove resistance discharging points should always be used.

In concluding this paper it is proper to say that I have attempted to consider the subject of lightning rods simply in its practical bearing. It has been not so much my object to present new facts or theories, as to urge a better application of principles well known,—to point out some of the most prominent defects in the construction of rods, and finally to present such obvious means of removing these defects as observation and experiment seemed to suggest. Neither have I pretended to give a full and detailed description of the rod. Nothing has been said of its height above the roof; the number of points at the tip; the mode of insulating and fastening to the building; of the general style, form, size or material, simply because most of these are deemed of minor importance, and the great defects, which it has been the chief object to bring out, are not here found.

V. CHEMISTRY.

1. On Molecular Perturbations.* By Gustavus Hinrichs, of Iowa City, Iowa.

Any series of compounds of like structure may be written

$$RM \qquad (1)$$

where M represents that part of these compounds which is common to all members of the series, while R represents the *radical* changing from one compound to the other. Thus in the nitrates of monatomic radicals

$$R' O_3 N$$

we have

$$R = R' \text{ and } M = O_3 N.$$

By an application of the general mechanical principles of the

* Also No. 3 of my *Contributions to Molecular Science.*

fourth section of my *Atomechanik* (1867), the *normal form* of the atomic structure, RM, can be determined. Already in that work it *has* been done for certain series, like the Tritoids AB_2, the Deltoids, AB_3C, etc.

Let the axes of this normal form, or the *normal axes* of the compound atom, RM, be

$$x: y: z \qquad (2)$$

These *normal* axes correspond to a given ideal case; just as the *normal* elliptical orbit of the planets corresponds to the ideal case of but one planet encircling the sun, and both bodies being infinitely small in comparison to their mutual distance. The normal axes correspond to *mutually equal* atoms (*Atomechanik*, §. 231 and §. 233).

Accordingly there always must be a certain value of R for which the given M will produce exactly the normal axes in the compound RM. Let this value of R be represented by r. Then the deviation of the *actual* axes

$$a: b: c \qquad (3)$$

from the normal axes must be proportional to the difference $R-r$. These deviations we call *molecular perturbations*, and denote them *in general* by π; in the direction of x, y, z, they are denoted, respectively, by ξ, ζ, ζ.

If, then, k be a *constant*, mainly determined by M, we shall have, in general, the molecular perturbation

$$\pi = k\ (R-r) \qquad (4)$$

A difference, $R-r=1$, gives $\pi=k$; that is k *is the perturbation due to each unit in the atomic weight of the disturbing element* R.

From this it is also apparent that the greater the disturbed mass M the smaller must be the coefficient k; or, approximately for comparable cases,

$$k = U - VM \qquad (5)$$

where U and V are constants.

It is admitted that the preceding deductions are in a more popular form; those who ask for more formal mathematical developments are referred to the latter part of this paper. In order not to repel the chemist and mineralogist, the more special developments were deferred to this place.

The value of r is also determined mainly by M. Thus if RM represents compounds, resulting by synthesis of R and M under great evolution of heat, a considerable attraction or affinity between R and M is manifest. Accordingly r will be negative, indicating that it would require a *repulsion* of the numerical value r to produce the normal equilibrium or form.

We may now proceed to the application of these principles.

I. Titanoid Dioxides, $TτO_2$.

Referring to contribution No. 1 (p. 13)* for the details, we have, agreeable to (4):

$$\zeta = -0.001\,[Tτ + 130] \qquad (6)$$

which gives very good results. Accordingly the attraction between the titanoid, $Tτ$, and the two atoms of oxygen is so great that a repulsion of 130 ($r = -130$) would be required to produce the normal form.

The normal form of these titanoid dioxides is

$$x:y:z = \sqrt{3} : 1 : \sqrt{3}$$

(See Contrib. No. 1, p. 11, or Report Chicago Meeting, p. 217). The perturbation ζ is in the axis z, which passes through the atom $Tτ$ and stands at right angles to the middle of the line joining the two atoms of oxygen. The observed axes a, b, c, tabulated at the close of this paper, give the *observed* perturbations by means of

$$a = x + \xi \qquad b = y = 1, \qquad c = z + \zeta \qquad (7)$$

For these dioxides the values are

	$Tτ$	ζ *calculated.*	*observed.*
Cassiterite	$Sn = 118$	-0.248	-0.245
Rutile	$Ti = 50$	-0.180	-0.180
Zircon	$\frac{Zr + Si}{2} = 59$	-0.189	-0.171
Pyrolusite	$Mn = 52$	-0.182	-0.180

Although manganese is not a titanoid, the perturbation of pyrolusite very nearly agrees with that calculated for titanoid-

* Report of Chicago Meeting (1868), p. 221.

dioxides. The calculated value for Zircon differs considerably from observation; it will necessarily depend upon the exact formula of the mineral, while the calculated value above given is obtained on the hypothesis of an equal number of atoms of zirconium and silicon. Thus

$$\frac{Zr + 3\,Si}{4} = 43.5$$

would give —0.173 instead of the —0.171 observed.

II. Aragonitoid Carbonates.

The three atoms of oxygen in the Carbonates $R''\,O_3\,C$ determine the plane $y\,z$, the axis z passing through one of these atoms, while y is at right angles thereto in the centre of gravity of the triangle formed by the three oxygen atoms. This triangle is nearly or exactly equilateral, giving the normal axes

$$y : z = 1 : \sqrt{3}$$

The axis x is at right angles to both, and passes in one direction (say upwards) through the Calcium atom, in the other direction through the Carbon atom, so that the angle

$$Ca - O - C \text{ is } 90°$$

or very nearly so. Thus $x = z = \sqrt{3}$.. See *Atomechanik*, §§ 303–306.

The observed values of a, b, c, tabulated at the close of this paper, give by (7) the following values of the *perturbation* ζ *in the oxygen plane* after axis Z.

	Xa		ζ *calc.*	ζ *obs.*	*calc.–obs.*
Aragonite	Ca	= 40	—0.127	—0.1266	0.000
Strontianite	Sr	88	—0.085	—0.090	+0.005
Witherite	Ba	137	—0.040	—0.040	0.000
Cerusite	Pb	207	+0.024	—0.093	+0.117

The calculated values have been obtained according to (4) by

$$\zeta = 0.0009\ (Xa - 181) \qquad (8)$$

It is seen that lead, not being a calcoid, but member of the genus cadmoids, the corresponding carbonate does not agree in its perturbation with the calcoid-carbonates, thus proving that the cadmoid-atoms, in form or structure, essentially differ from the calcoid atoms, a result which must be admitted also on other grounds. Thus the calcoids are non-volatile, while the cadmoids are comparatively very volatile metals.

Applied to the perturbation of the *vertical prism* the general law (4) gives

$$\pi = 0'.72\ (Xa - 200) \qquad (9)$$

		Xa	τ *calc.*	τ *obs.*	*calc.-obs.*
Aragonite	$Ca =$	40	$-114'.2$	$-115'$	$+\ 0'.8$
Strontianite	Sr	88	$-\ 80'.6$	$-\ 80'$	$-\ 0'.6$
Witherite	Ba	137	$-\ 44'.4$	$-\ 45'$	$+\ 0'.6$
Cerusite	Pb	207	$+\ \ 6'.$	$-\ 83'$	$+89'.$

The normal angle between the vertical prisms and the plane yx is $p = 60°$. The above minutes represent the derivation of the observed values from this angle of 60°.

The value of r is *positive* in both formulæ (8 and 9) but not quite identic, although it ought to be so. It requires an attraction of 181 according to (8), and of 200 according to (9), to reduce the perturbation in the oxygen section to nothing; that is, to make the aragonitoid hexagonal for the calcoid-elements Xa.

I think that this is in accordance with chemical principles. The oxygen of the electro-positive $Xa\,O$, being in the same plane with the oxygen of the electro-negative $C\,O_2$, exerts a repulsion on the same, equivalent to $r = 181$. Compare §315 of *Atomechanik* on this point. In the hexagonal Calcit-forms, the $Xa\,O$ and $C\,O_2$ exist together as *one* $Xa\,O_3\,C$, and accordingly $\zeta = o$. Herewith harmonizes the fact, that according to *Favre* and *Silbermann* (1852), each gram of aragonite sets free 39.1 calorics when being converted into calcite; or each atom $Ca\,O_3\,C$ sets free 3910 calorics. Compare also VI. in Contribution 5.

III. CALCITOIDS.

Of the carbonates, only magnesite and smithsonite belong to the same genus. This is sufficient to determine the two constants in (4), but not enough to give any control. The three aragonitoid carbonates have, however, given that control, so that we may apply (4) also on the calcitoids.

Indirectly we shall have a very excellent control in the values of the constants thus determined; for they must agree with the known chemical properties of the elements.

For the calcitoids the horizontal perturbations ζ is zero; the vertical ξ is for the cadmoids.

$$\xi = -0.1092 - 0.000161\, K\delta \qquad (10)$$

or

$$\zeta = -0.000161\, [K\delta + 678] \qquad (11)$$

The same perturbation is, for the angle R of the rhombohedron, the normal being 104° 29′ (Atomech., §307)

$$\rho = +0.'25\, (K\delta + 698) \qquad (12)$$

The observed values are:

	$K\delta$	R	ρ	ξ
Magnesite	*Mg*, 24	107°.29	+180′	−0.1131
Smithsonite	*Zu*, 65	107°.40	+191′	−0.1197

The value of $r = -678$ or -698 indicates an *attraction* of nearly 700 between *RM*, or a repulsion of this magnitude is required in order that $K\delta\, O_3\, C$ may have the normal form, the rhombohedron $R = 104°\ 29'$. Now this is exactly what might have been predicted, since the cadmoids have not only a great affinity for *O*, but also because their atomic weight is in excess of both *O* and *C*. Thus, in Smithsonite, the amount of matter in any horizontal is equivalent to $2\,O = 32$, but in the vertical it is $C + Zn = 12 + 65 = 75$, or more than twice as much. Since there is no chemical repulsion between Carbon and the Cadmoids, this preponderance of matter in the vertical axis must contract the same, as observed.

The two sulpho-salts $Ag_3\, S_3\, \Phi$ of this form give for (4):

$$\xi = -0.00066\, [\Phi + 114] \qquad (13)$$

and

$$\rho = +1'.15\, [\Phi + 98] \qquad (14)$$

the observed values being

	Φ	R	ρ	ξ
Proustite	$As =$ 75	107°48′	+199′	−0.125
Pyrargyrite	$Sb =$ 122	108°42	+253′	−0.156

The normal rhombohedron of 104° 29′ will thus belong to a phosphoid Φ of the atomic weight $r = -98$ or -114; that is a *repulsion* of from 100 to 120. This is also in accordance with the chemical properties of the elements here involved. For the phosphoid and silver, here in the axis X, very readily combine, thus indicating an attraction of Ag_3 or Φ, so that for any Φ it would require a *repulsion* to produce the normal form. The same follows by considering the mere mass in the different directions; thus along Z and Y we have the equivalent of $2\,S = 64$, but along X we have $3\,Ag + \Phi = 324 + \Phi$ which for Proustite is 399; for Pyrargyrite 446, that is from 6 to 7 times as much!

IV. Calcoid Sulphates.

In the sulphates $S\,O_2$ exists as such, forming, like all compounds of the formula $A\,B_2$ or $A \left\{ \begin{smallmatrix} B \\ B \end{smallmatrix} \right.$, an equilateral triangle, either exactly or nearly so. The axis Z is laid through S, and Y through the two atoms of oxygen of $S\,O_2$. The axis X is at right angles to these, and contains the other two oxygen atoms of the sulphates. As demonstrated in another place, the *normal* axis of the sulphates $R''\,O_4\,S$ are

$$x : y : z = \sqrt[4]{3} : 1 : \sqrt[2]{3} \qquad (15)$$

The perturbations most directly due to the different radicals are those in the axis of Z; for the radical R'' is directly opposite to the sulphur-atom, S. The normal vertical prism, parallel to the axis X, is nearly *hexagonal;* the normal angle between the vertical face and the plane $X\,Y$, is 60°; the deviation of the crystal from this angle we call π. The observed values then give for these perturbations for the calcoids:

$$\zeta = -0.0008\,[Xa + 20] \qquad (16)$$

$$\pi = -0'.7\,[Xa + 20] \qquad (17)$$

Name.	Xa	ζ calc.	obs.	π calc.	obs.
Anhydrite	Ca 40	—0.048	—0.048	— 42′	— 42′
Celestite	Sr 88	—0.086	—0.089	— 75′.6	— 80′
Barite	Ba 137	—0.126	—0.122	—109′.9	—110′

Accordingly, $r = -20$ in both of these equations. That is, it requires a repulsion of 20 to produce the normal hexagonal form of the vertical prism here under consideration. This is again in harmony with the atomic structure and the known affinities. For in the axis Z we have $S + Xa = 32 + Xa$, reaching from 72 (for Ca) to 169 (for Ba), while in the axes X and Y there are only 2 atoms of oxygen, weighing 32. Hence a greater attraction in the direction of the Z axis, or a shortening of the same (ζ negative, r negative). Besides, we know that the calcoids have a considerable affinity for sulphur—which, therefore, increases the perturbation due to the mass merely.

This law has been extended to Selenates, Chromates, Perchlorates, Permanganates (see Synopsis of new Memoirs on *Atomechanics*, published July, 1868). Here it may be sufficient to give the observed values of the angular perturbation π, or the deviation of the vertical prism found forming 60° with the vertical plane xy.

Name.	*Formula.*	π	
Anglesite	$Pb\,O_4\,S$	— 81′	
Barite	$Ba\,O_4\,S$	—110′	mean — 77′ (Barite, Celestite, Anhydrite)
Celestite	$Sr\,O_4\,S$	— 79′	
Anhydrite	$Ca\,O_4\,S$	— 42′	
Permanganate	$Ka\,O_4\,Mn$	— 92′	Mitscherlich.
"	"	— 80′	Groth.
Perchlorate	$Ka\,O_4\,Cl$	— 86′	Groth.
"	"	— 83′	Mitscherlich.
Olivenite	$Mg_2\,O_4\,Si$	— 24′	Haidinger.
Phenacite	$Be_2\,O_4\,Si$	— 0	——
Willemite	$Zn_2\,O_4\,Si$	+ 0	——
Normal form	$A\,B_4\,C$	0	——
Sulphate	$Ka_2\,O_4\,S$	+ 12′	Mitscherlich.
Selenate	$Ka_2\,O_4\,Se$	+ 12½	"
Chromate	$Ka_2\,O_4\,Cr$	+ 20′	"
Manganate	$Ka_2\,O_4\,Mn$	+ 35′	"
Ammonium Chloro Zincate	$Am\,Cl_4\,Zn$	+ 40′	

The most interesting fact exhibited in these perturbations is the change of π by the doubling of Ka. A mere increase in weight of Ka in the permanganate $Ka\ O_4\ Mn$ would increase the negative value of π. But the addition of another atom of potassium, producing the Manganate $Ka_2\ O_4\ Mn$, does *not* increase the perturbation in the same direction, but changes its sign. This indicates a slight but real repulsion between the two atoms of potassium, in harmony with the electro-chemical views.

V. Perturbations and Temperature.

On page 43 of *Atomechanik*, the influence of temperature on the molecular perturbations has been investigated, and the results exemplified by means of *Fizeau's* observations then published. In Pogg. Ann. 1868, p. 135, we find additional observations of *Fizeau*, so that we now can continue the investigation of the subject.

In the following determinations of *Fizeau* the coefficient of expansion for one degree centigrade (at the temperature of 40°), is in the direction of (Fizeau's) vertical axis a, at right angles thereto a'; and the change of either for each degree of temperature is Δa and $\Delta a'$. For the sake of simplicity we refer all of these numbers to the original length of *one million units;* then $a = 0.00001911$ of *Fizeau* for Fluorspar (the expansion of the length 1) becomes here $a = 19.11$ (the expansion of 1,000,000).

Tritoids, AB_2.

$CaFl_2$, Fluorspar	$a = a' = 19.11$	in all directions.
Fe_2S_2, Pyrite	$a' = a' = 9.08$	
Cu_2O, Cuprite	$a = a' = 0.93$	
"	$\Delta a = \Delta a' = 0.0210$	

	a	a'	Δa	$\Delta a'$
Quartz, SiO_2	7.81	14.19	0.0177	0.0238
Rutile, TiO_2	9.19	7.14	0.0225	0.0110
Cassiterite SnO_2	3.92	3.21	0.0119	0.0076
Zircon (Si, Ti) O_2	4.43	2.33	——	——

Deltoids, AB_3C.

For these a is the expansion in the direction of our axis X, or a, a' in the direction Y or b, and a'' in the direction Z or c.

	a	a'	a''
Calcite	+ 26.21	— 5.40	— 5.40
Araganite	+ 34.60	+ 10.16	+ 17.19

These values must be referred to $b = 1$, by subtracting the coefficient after this axis from each of the other coefficients; the values thus obtained express the relative expansion in these axes. It must also be borne in mind that these coefficients refer to our X, Y, Z, in the following manner:

	X	x	Y	y	Z	z
Quartz	a	1	a'	$\sqrt{3}$	a'	$\sqrt{3}$
Rutile	a'	$\sqrt{3}$	a	1	a'	$\sqrt{3}$
Cassiterite	a'	$\sqrt{3}$	a	1	a'	$\sqrt{3}$
Zircon	a'	$\sqrt{3}$	a	1	a'	$\sqrt{3}$

The values of our *normal axes* x, y, z, have also been added. For details see *Atomechanik*, p. 34, or Contributions to Molecular Science, No. 1 (Report Chicago Meeting, p. 217).

This gives our $y = b = 1$, with no relative expansion and no perturbation. For the other axes we obtain: $\delta\xi$ as the relative coefficient of expansion after X (for each million of *units* of length), $\delta'\xi$ the variation of $\delta\xi$ for each degree; and $\delta\zeta$, $\delta'\zeta$ for the axis Z. The results are contained in the following table:

THE TESSERAL TRITOIDS.

Fluorspar, *Pyrite* and *Cuprite*

have $\xi = \zeta = o$; and also $\delta\xi = \delta\zeta = o$.

THE HEXAGONAL TRITOID.

Quartz	$x = 1.0000$	$z = 1.7320$
	$\xi = +0.099$	$\zeta = 0.0000$
	$\delta\xi = -6.38$	$\delta\zeta = 0.0000$
	$\delta'\xi = -0.0061$	$\delta'\zeta = 0.0000$

THE QUADRATIC TRITOIDS

have $x = z$ and also their variations equal, so that *they remain quadratic.* We give only the values for x:

	$x = z = \sqrt{3}$	$\xi = \zeta$	$\delta\xi = \delta\zeta$	$\delta'\xi = \delta'\zeta$
Rutile	1.7320	—0.180	+2.05	+0.0115
Cassiterite	1.7320	—0.245	+0.71	+0.0043
Zircon	1.7320	—0-171	+2.10	+ ——

THE HEXAGONAL DELTOID.

Calcite	$X = \sqrt{3} = 1.7320$	$z = \sqrt{3} = 1.7320$
	$\xi = -$ 0.0235	$\zeta =$ 0.0000
	$\delta\xi = +$ 31.61	$\delta\zeta =$ 0.0000

THE RHOMBIC DELTOID.

Aragonite	$X = \sqrt{3} = 1.7320$		$z = \sqrt{3} = 1.7320$	
	$\xi = +$	0.0035	$\zeta = -$	0.1266
	$\delta\xi = +$	24.44	$\delta\zeta = +$	7.03

These observations show:

1. That *when the molecular pertubation is zero, it remains so.* In other words, the normal form is not changed by an increase of the temperature.—Examples: the tesseral tritoids, also ζ in quartz and calcite.

2. That *the molecular perturbations are diminished by increase of the temperature.* In other words, the actual form approaches the normal form when the temperature is raised. Examples are quite numerous in the above observations; they are indicated by the opposite sign of the perturbation and the coefficient of expansion.

3. The expansion of aragonite in the direction of the vertical axis X is an exception to the preceding rule; for ζ and $\delta\zeta$ have the same sign, so that the actual form passes farther and farther from the normal as the temperature is raised. This proves that the atoms in aragonite are not in a stable equilibrium; and in fact, by heating a crystal of the same, it suddenly changes from aragonite to calcite at a certain temperature, developing 3910 calorics (for each atom of C_2O_3C) in the process (*Favre* and *Silbermann*, 1852).

Thus we have brought all of the preceding determinations of the expansion of crystals under the one general law; *diminution of the molecular perturbation to zero*, or approximation of the crystal to the normal form.

The preceding, we hope, is sufficient to show that the *molecular perturbations* are a reality, as well as the cosmical perturbations of astronomy. However incomplete and imperfect our knowledge of this subject may be at present, we deem it sufficient to invite to farther research. While we believe the *general law* (4) of molecular perturbations to have been sufficiently proved by observations, we admit that the precise value of the constants k and r is at present only imperfectly determined by observation, and only in a very general way by theory.

The exact theoretical determination of these constants is a mechanical problem, the solution of which depends as much upon the constitution and form of the atoms of the so-called chemical elements, as does the theoretical determination of cosmetical perturbations on the structure, form and mass of the cosmical bodies.

APPENDIX.

That the law (4) is in fact a first approximation is proved by the following preliminary analysis of this problem, referred to in the first part of this paper.

Let the substance under consideration be a ternary, composed of the element-atoms A, B, C. To simplify the subject, let the compound be a deltoid, AB_3C.

The actual axes of this deltoid we call a, b, c; the line joining the two atoms A and C, being taken as X-axis or its value $= a$.

If now, without changing anything, the atoms A and C were made equal to B, the axes a, b, c, would become x, y, z, *the normal axes*. Leaving C, but substituting only the radical $A = R$, a certain value $R = r$ must likewise produce the normal axes x, y, z; the corresponding compound being $r\ B_3C$. Accordingly the force producing increments ξ, y, ζ, on x, y, z, is *zero* for $A = R = r$.

Now, let the force between A and C be represented by the function $\Psi\ (A, C)$; similarly that of A and B by $\varphi\ (A, B)$, between C and B by $\varphi'\ (C, B)$, and the repulsion between the like atoms B by $p\ (B, B)$. Into all of these the actual axes a, b, c enter as variables. The resulting force, therefore, will be

$$F\ (A', B, C) = \Psi\ (A, C) + \varphi\ (A, B) + \varphi'\ (C, B) - p\ (B, B), \tag{18}$$

By substituting $A = r + h$ and developing we obtain

$$F(r + h, B, C) = K_0 + K_1 (A - r) + K_2 (A - r)^2 + K_3 (A - r)^3 + \text{etc.} \tag{19}$$

where

$$\left.\begin{aligned} K_0 &= \Psi(r, C) + \varphi(r, B) + \varphi'(B, C) - p(B, B) \\ K_1 &= \frac{d\Psi(r, C)}{dA} + \frac{d\phi(r, B)}{dA} \end{aligned}\right\} \tag{20}$$

The functions, Ψ, φ, etc., contain probably only the first powers of the atomic weights; hence K_2, K_3, etc., will be zero, or the squares and higher powers of $(A-r)$ may be omitted.

At the same time we have, according to the definitions given

$$a=x+\xi \qquad b=y+\zeta \qquad c=z+\zeta \tag{21}$$

(See also 7).

If, therefore, we neglect the square and higher powers of $(A-r)$, we have for a *first* approximation, the normal axes determined by K_0, and the perturbations by K_1. For the perturbations π in *any* direction we shall have an equation of the

$$\pi=K_1\,(A-r) \tag{22}$$

form which is identic with (4). It is evident that K_1 depends upon the normal axes, x, y, z, and the values of r, B and C. If the form of the functions, as well as the normal axes are known, then K_0 would serve to determine the value of r.

The above formulæ have been simplified by not writing the values of a, b, c, which enter into every one of functions. Since now the law of the variation of these attractions and repulsions in regard to distance and angle (atomicity) is unknown, we may be excused from entering into any farther detail. At the same time it will be admitted, that the only possible solution is based upon an accurate knowledge of the *element-atoms as physical bodies*, such as they are represented in the first section of my *Atomechanik.*

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2. On the Classification and the Atomic Weights of the so-called Chemical Elements, with reference to Stas' Determinations. By Gustavus Hinrichs, of Iowa City, Iowa.

Like all other natural objects the chemical elements (so-called) can be classified into *natural* groups, — *Genera* made up of the individual elements as *Species* and *Varieties.* Such a classification was published in 1867 in my *Atomechanik* (§ 17, pp. 7, 8).

The symbol of the *species* is the characteristic letters of the

Latin name of the same, introduced by *Berzelius* in 1815; it has also a *numerical* value, representing the weight of the atoms.

As symbol of the *genus* I introduced the characteristic letters of the *Greek* name of the same, and stated that this symbol represents an *equation*, the numerical value of which for the given variable would be the atomic weight of the species corresponding to that variable.

The theoretical ground of this classification is exposed in *Atomechanik.* We shall here *exclusively* consider this question from a practical and empirical point of view, entirely independent of the pantogen-hypothesis. In this form it has now for two years been presented in my lectures.

I. Classification of the Elements.

This *empirical* classification (in its results *identical* with that of *Atomechanik*) is based mainly on the deportment of the elements toward *heat.* This physical agent being motion, *we accordingly classify the elements in regard to the mobility of their particles* (*the atoms*). We distinguish *six* degrees of *fusibility*, *three* degrees of *volatility*, and the *coloration of the flame* for the metals. We retain the general division of the elements in two *orders*, metals and metalloids, based upon the presence or absence of metallic lustre. The above degrees of fusibility correspond very nearly to those of *von Kobell.*

For farther explanation we may refer to the following *table*, merely adding, that the *name* of the genus was taken from the most common or most characteristic element. The semi-barbarous name Sulphoid was chosen, because the more correct Thionoid is not so palpable to students.

GENERAL TABLE OF THE GENERA OF THE ELEMENTS.

I. METALS; having metallic lustre.

Fusibility.	Volatile.	Flame.	Genus. Name.	Genus. Symbol.	Representative Species.
1	exceeding	color	*Kaloids*	Κα	*Ka*
2	very	no color	*Cadmoids*	Κδ	*Cd*
3	slightly	——	*Cuproids*	Κυ	*Cu*
4	not	color	*Calcoids*	Χα	*Ca*
5	not	no color	*Ferroids*	Σι	*Fe*
6 (infus)	not	——	*Titanoids*	Ττ	*Ti*

II. METALLOIDS; not having metallic lustre.

	Name.	Symbol.	Representative Species.
Triatomic Elements like Phosphorus	*Phosphoids*	Φ	*P*
Di— " " Sulphur	*Sulphoids*	Θ	*S*
Mon— " " Chlorine	*Chloroids*	Χ	*Cl*
Mon— " " Pantogen	*Pantoids*	Υ	*H*

To these genera several others will have to be added; but not until the elements concerned are better known. Of such genera I mention here the Thalloids, $\theta\lambda$, Molybdoids, $M\lambda$, Hydrargoids, $\Upsilon\gamma$ from *Atomechanik*. Beryllium is either the first species of the Ferroids or of the Cadmoids.

The Ferroids are distinguished for their *varieties*. Thus the third species (Iron; Greek *Sideros*) is associated in nature and in its properties with four varieties, which we term the *Sideroids* and give the symbol $\Sigma\delta$. They are Chromium, Manganese, Iron (*the* species), Nickel and Cobalt. They form a regular series, connecting Titanium of the Titanoids with Copper of the Cuproids (see Chart). The Rhodoids and Iridoids are not sufficiently known yet. Uranium is associated with Cobalt and the Sideroids, having exactly twice the atomic weight of Cobalt.

The species in each genus are again arranged in the order of their volatility, *or* specific gravity, *or* atomic weight. By printing their symbols at distances from that of the genus, nearly proportional to the atomic weight, we obtain the following chart representing our classification of the elements. A few of the most important elements not embraced in the above ten genera have been added.

HINRICHS' CLASSIFICATION OF THE ELEMENTS.

GENERA.		Species.							
x =		1	2	3		4		5	
Υ		*H*							
Κα		*Li*	*Na*	*Ka*		*Rb*			
Χα		—	—	*Ca*		*Sr*		*Ba*	
Κδ		—	*Mg*		*Zn*		*Cd*		*Pb*
Υγ		—	—		—		—		*Hg*
Κυ		—	—		*Cu*		*Ag*		*Au*
Σι			*Al*	*Co* *Ni* *Fe* *Mn* *Cr*			*Rh*		*Ir*
Ττ		*C*	*Si*	*Ti*		*Pd* *Sn*		*Pt*	
Φ		*N*	*T*	*As*		*Sb*		*Bi*	
Θ		*O*	*S*	*Se*		*Te*		—	
Χ		*Fl*	*Cl*	*Br*		*Io*		—	
Υ	*H*								

That this is a truly *natural classification* is proved by the fact that in this table the elements of like properties, or their compounds of like properties, form groups bounded by *simple lines*. Thus a line drawn through *C*, *As*, *Te*, separates the elements; having metallic lustre from those not having such lustre. The gaseous elements form a small group by themselves, the condensible chlorine forming the boundary. So also the boundary line of the *heavy metals* (specific gravity above five) is a simple line, running between *Kδ* and *Xα*, as far as the break in the vertical columns goes (to *Zn*), then down to *Ti* and through *Se* out between *Te* and *Io*. So also the boundary lines for other properties may be drawn.

Of great practical importance are the lines expressing certain properties of definite compounds. Thus, the solubilities of Ternaries or Binaries in water. The reactions in the wet way (Fresenius). The blowpipe reactions, may be represented on such charts by means of a few simple lines. Such charts have been in use in my laboratory, and were exhibited to the Association — the symbols printed on muslin (size 8×6 decimeters) while the other details had been entered by hand. In the lecture room I also have been using a square blackboard of black walnut (one meter each way) with the symbols painted; a chalk line or figure then enters the property dwelt upon by the lecturer.

II. The Atomic Weights.

If we inscribe the atomic weight in the table of elements just given, it will immediately be seen that in each of the columns, marked $x = 1, 2, 3$, etc., comparatively small changes take place. This suggests a *general law, determining the atomic weight y of any species as a function of x for all genera.*

$$y = f\ (x). \tag{1}$$

Theoretically we may determine the nature of this function in the following manner: if we assume the existence of but one substance (pantogen).

A certain number c of the *mutually equal atoms* of this pantogen may combine, producing an element of the atomic weight $y = c$, that of hydrogen being one. Any two of these new atoms may again combine, giving $y_2 = 2c$. These again combining give $y_3 = 2y_2 = 2.2c$, of which again $y_4 = 2y_3 = 2.2.2c$ would follow. Thus *by simple reduplication* of the original atoms $y_1 = c$, we would obtain atoms of other substances (elements) expressed by the general formula

$$y = c.2^{x-1} \qquad (2)$$

All of the elements for which c has the same numerical value would evidently closely resemble one another, or constitute a *genus* of elements. Variation in the value of c would give the different genera.

Another mode of combination in the successive addition of the same c to itself, giving

$$y = c.\,x \qquad (3)$$

There are evidently other methods of combination; but those here given are undoubtedly the *simplest possible*, and hence the most probable.

A parallel to the law (3) we have in the hydrocarbons, where the successive atoms of carbon are linked together by means of hydrogen atoms. Accordingly it is possible, and rather probable, that less complex aggregations k of atoms of the primary matter will be required as links to bind together the members c of the atoms y. Consequently a more general form of the preceding laws would be

1. *For Reduplication*:

$$y = c.\,2^x + k \qquad (4)$$

2. *For Simple Aggregation*:

$$y = c.\,x + k \qquad (5)$$

In their equations k must necessarily be *comparatively small* if it changes with x, otherwise expressive of a terminal common to the genus, it may be even large, as in the Cuproids, Cadmoids and Calcoids. From the observed values of the atomic weights *we may conclude that all the metallic elements, except the calcoides, are formed by reduplication* (4); *the calcoids being formed by aggregation* (5). *The metalloids are formed mainly by reduplication* (4).

This we conclude from the following comparison between the calculated and observed values of the atomic weights. In all of the following tables, the *correction* is to be added to the calculated value in order to give the observed value, taken from Jahresbericht, 1866.

A. METALS.

1. *Genus.* Kaloids.

$$Ka = 5.2^x + 3\,(-1)^x \tag{6}$$

Species.	x.	Ka calc.	Ka obs.	Correction.
Lithium	1	7	7	0
Sodium	2	23	23	0
Potassium	3	37	39.1	+2.1
Rubidium	4	83	85.4	+2.4
. . . .	5	157	—	—

2. *Genus.* Cuproids.

$$K\upsilon = (5.5)\,2^x + 20 \tag{7}$$

. . . .	1	31	—	—
. . . .	2	42	—	—
Copper	3	64	63.4	−0.6
Silver	4	108	108	0
Gold	5	196	197	+1

3. *Genus.* Cadmoids. Omit the constant for $x = 1$ and 2.

$$K\delta = 6.2^x + 16 \tag{8}$$

. . . .	1	12	—	—
Magnesium	2	24	24	0
——				
Zinc	3	64	65.2	+1.2
Cadmium	4	112	112	0
Lead	5	208	207	−1

4. *Genus.* Ferroids.

$$\Sigma\iota = (6.25).2^x + x\,(+1)^x \tag{9}$$

——	1	13.5	—	
Aluminium	2	27	27.4	+0.4
Iron	3	53	56	+3
Rhodium	4	104	104.4	+0.4
Iridium	5	205	198	−7

5. *Genus.* Titanoids.

$$T\tau = (6.5).2^x + x\,(-1)^x \qquad (10)$$

Carbon	1	12	12	0
Silicon	2	28	28	0
Titanium	3	49	50	+1.0
Palladium	4	108	106.6	−1.4
Platinum	5	203	197.4	−5.6

6. *Genus.* Calcoids.

$$Xa = 48\,(x-2) - 8$$
$$= 48\;x - 104 \qquad (18)$$

Calcium	3	40	40	0
Strontium	4	88	87.6	−0.4
Barium	5	136	137	+1

B. Metalloids.

For these we have in general

$$\left.\begin{array}{l} y = c.\;2^x \\ y = c\,[2^2 + b.\;2^{x-2}] \end{array}\right\} \quad (11)$$

where the upper is to be used for $x = 1, 2$; the lower for $x = 3$, 4, 5, the sum of the exponents always being $= x$. For the Sulphoids and Phosphoids this discontinuity is expressed in the first corresponding strictly to metalloids, while the latter ($x = 3$) have metallic properties. The factors multiplying c will in all cases make y_3 the arithmetical mean between y_2 and y_4,

$$2\,y_3 = y_2 + y_4 \qquad (12)$$

as readily may be seen from (11).

We obtain again for x, y calculated, y observed, and the "correction," the values tabulated below.

For the sake of abbreviation, the function of x multiplying c is denoted by X, so that

$$\left.\begin{array}{lll} \text{for } x = 1, 2 & & X = 2^x \\ \text{"} \quad 3, 4, 5 & & X = 2^2 + 6.\;2^{x-2} \end{array}\right\} \quad (13)$$

For the phosphoids and sulphoids, we have $b = 3$, for the chloroids $b = 2.5$. Hence we find

$$\left.\begin{array}{ll} \text{for } \Phi \text{ and } \Theta, & \tfrac{1}{2}X = 1, 2, 5, 8, 14 \\ \text{"} \quad X & \tfrac{1}{2}X = 1, 2, 4._5, 7, 12 \end{array}\right\} \quad (14)$$

7. *Genus.* PHOSPHOIDS. $b = 3$.

$$\Phi = 7.5\,X \qquad (15)$$

$$\tfrac{1}{2}\,X = 1,\ 2,\ 5,\ 8,\ 14 \text{ (see 14).}$$

Nitrogen	1	15	14	−1
Phosphorus	2	30	31	+1
Arsenic	3	75	75	0
Antimony	4	120	122	+2
Bismuth	5	210	210	0

8. *Genus.* SULPHOIDS. $b = 3$, $\tfrac{1}{2}\,X = 1, 2, 5, 8, 14$.

$$\theta = 8.X \qquad (16)$$

Oxygen	1	16	16	0
Sulphur	2	32	32	0
Selenium	3	80	79.4	−0.6
Tellurium	4	128	128	0
——	5	224	——	——

9. *Genus.* CHLOROIDS. $b = 2.5$. $\tfrac{1}{2}\,X = 1, 2, 4\tfrac{1}{2}, 7, 12$.

$$X = 9.X \qquad (17)$$

Fluorine	1	18	19	+1
Chlorine	2	36	35.5	−0.5
Bromine	3	81	80	−1
Iodine	4	126	127	+1
——	——	216	——	——

We shall not discuss the "correction" which is to be applied to the calculated values in order to give the observed values. We only shall observe that these corrections are very small except for the three species *Fe*, *Ir*, *Pt*, which belong to genera known to contain varieties.

We do not mean to have the observed values corrected, for what here appears as "corrections" may in fact represent the links which hold together the various portions of the resulting atom. A negative correction would thus indicate that some projecting point had been removed before combination was effected.

Nor do we assert that the atoms of the so-called elements have a composition expressed by the general formula (4) (and its modification (11) for the metalloids); *but their atomic weights do make such composition probable*, while at the same time *such constitution would be the simplest possible if the elements were composed of one primary matter* (*pantogen*).

At all events it is thought that the relations here pointed out are of a more general and more rational character than those published by *Dumas* * in 1857.

In conclusion we give a synopsis of the formula represented by our generic symbols:

$$\text{Genus in general } y = c.2^x + k \qquad (4)$$

Kaloids	$Ka = 5.2^x + 3\,(-1)^x$	(6)
Cuproids	$K\upsilon = (5\tfrac{1}{2}).2^x + 20$	(7)
Cadmoids	$K\delta = 6.2^x + 16$	(8)
Terroids	$\Sigma\iota = (6\tfrac{1}{4}).2^x + x\,(+1)^x$	(9)
Titanoids	$T\tau = (6\tfrac{1}{2}).2^x + x\,(-1)^x$	(10)

Phosphoids	$\Phi = (7\tfrac{1}{2}).\,[2^2 + 3.2^{x-2}]$	(15)	Sum of exponents is x. For $x=1$, 2 the parenthesis is 2^x
Sulphoids	$\theta = 8\,[2^2 + 3.2^{x-2}]$	(16)	
Chloroids	$X = 9\,[2^2 + (2\tfrac{1}{2}).2^{x-2}]$	(17)	

Calcoids	$Xa = 48\,x - 104$	(18)

From this it will be seen that the volatility and fusibility of the metals decreases as the value of c increases. Their *atomicity* increases with increasing c. For the metalloids the reverse is the case, which fact probably is connected with the electrical contrast between these two orders of elements.

Graphical representation, taking x as abscissa and y as ordinate, will conduce very much to a better apprehension of the relations and the general formula here presented. When presenting this subject to the Association a diagram about $2\frac{1}{2}$

* L' Institut, 1857, pp. 420-422; pp. 383-386.

metres long was used; it contained the logarithmic or exponential curves for the Kaloids, Cuproids, Titanoids, Sulphoids and the straight line of the Calcoids. The unit of x was 18^{cm}, of y was 1^{cm}, so that $Bi = 210$ was represented by an ordinate of 210^{cm}.

III. Stas' Determinations.

Very much is said about these very interesting determinations of the atomic weight; but we believe that the true importance of these great labors is hardly estimated yet. Most chemists seem to think that the chief importance of the painstaking work of *Stas* is to disprove and forever reject the so-called hypothesis of *Prout;* and with the destruction of this hypothesis they seem to think all the palpable harmonies of the atomic weights, and particularly all relating to "*pantogen*" is annihilated. We are inclined to think that just such careful determinations *will demonstrate* the correctness of the law of a common divisor (equal one-half the atomic weight of hydrogen?) for all elements, *and* prove some essential features of the *structure* of the element atoms.

In order to approach this subject let us grant all that *Stas* does claim for his figures; that is, they are exact to the first or even the second decimals. *Let us see, what these figures, if they are so accurate as claimed, really do prove!*

The values of *Stas* are actually obtained for oxygen equal to 16. Let us represent these values by S. The atomic weights in accordance with *Prout's Law* (referred to one-half hydrogen) we shall for the sake of convenience call the *normal* atomic weights, A. The deviation of observation from these normal values we denote by D,

$$D = S - A \text{ or } S = A + D.$$

The deviation per unit of weight will then be

$$d = \frac{D}{A}$$

The values A, S, D and d are given in the following table, *S* being taken from *Fresenius' Zeitschrift für Analytische Chemie*, 1868, p. 169.

	A.	S.	D.	d.
KALOIDS, Ka, mean		. . .	. . .	+ 0.00284
Lithion, *Li*,	7	7.022	+ 0.022	+ 0.00314
Sodium, *Na*,	23	23.043	+ 0.043	+ 0.00187
Potassium, *Ka*,	39	39.137	+ 0.137	+ 0.00351
PHOSPHOIDS, Φ				
Nitrogen, *N*,	14	14.044	+ 0.044	+ 0.00314
PANTOIDS, Y				
Hydrogen, *H*,	1	1.002	+ 0.002	+ 0.00200
SULPHOIDS, Θ				
Oxygen, *O*,	16	16.000	± 0.000	——
CHLOROIDS, X, mean		. . .	. . .	— 0.0008
Chlorine, *Cl*,	35.5	35.475	— 0.025	— 0.0007
Bromine, *Br*,	80	79.952	— 0.048	— 0.00060
Iodine, *Io*,	127	126.850	— 0.150	— 0.00118
CUPROIDS, Kυ				
Silver, *Ag*,	108	107.930	— 0.070	— 0.00064

It is apparent that *d* is nearly constant for the same genus, and very nearly + 0.003 for the Kaloids, — 0.001 for the Chloroids. In other words the deviation — whatever be its cause — is nearly proportional to the absolute weight determined!

Another very peculiar fact is the *positive* sign of the deviations, *D* and *d*, for the elements which are electro-positive in regard to the standard (oxygen), while the deviations are *negative* for the elements which are electro-negative in regard to oxygen. Silver forms the only exception to this rule. At the same time its deviation *d* is only 0.0006, or much *less* than that of all other elements except bromine; and the electrochemical difference between silver and oxygen is not considerable either.

We conclude, therefore, that *Stas'* determinations *S* of the atomic weights deviate from the normal atomic weights *A* by small quantities *D* which

First, in regard to sign agree with the electro-chemical sign of the element in regard to oxygen (silver excepted); and

Second, are nearly proportional to the atomic weight itself.

Granted the accuracy of the determinations of *Stas*, the conclusions just given follow. We accordingly have a case in chemistry like that which astronomy would have offered if the perfection of the instruments and methods of observation had progressed more rapidly than theory, after the propounding of Kepler's laws. Then deviations from the elliptical orbits would have been observed, and it would have been discovered that these deviations were closely related to the relative position of the planets. If an expert observer on account of these deviations should have rejected *Kepler's* laws as disproved, he would have been greatly in error, because, as we now know, the observed deviations are consequences of the same law of gravitation on which *Kepler's* laws depend.

Now, theoretical chemistry is vastly behind practical (empirical) chemistry, in fact, so much behind this latter that the claims of mathematical chemistry are generally either ignored or derided. We shall, therefore, not venture to give our explanation of the deviations *d*; we probably only would deter from those much needed researches, which will demonstrate what we now must leave as merely probable.

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8. On the Grahamite of West Virginia, and the New Colorado Resinoid. By Henry Wurtz, of New York.

It is now nearly five years since my attention was first called to this remarkable mineral. I at once recognized the scientific interest that attaches to it, and have always earnestly desired to carry out some systematic and thorough chemical investigations into its nature and relations; a desire so far frustrated to a certain extent. Pending, however, renewed efforts which I am making, to accomplish complete elementary analyses of the constituents of Grahamite, as discovered by me, I think it my duty to communicate, for the first time, the full results of my former investigations.

Previous to 1865, the existence was frequently reported, and alluded to, in the public prints, of an extraordinary mineral formation on a branch of Hughes River, Ritchie County, West Virginia. It was sometimes called "Ritchie Coal," "Ritchie Asphaltum," "Crystallized (!) Petroleum," etc., etc. GESNER and others claimed it as identical with albertite, which they called an asphaltum (GESNER, *On Coal Oils*, *Petroleum*, *etc.*, p. 27).

The eminent geologist, Professor LESLEY, was the first to introduce its existence to the general knowledge of the scientific world, which he did in a communication to the American Academy in Philadelphia (in 1864, I believe); not having himself visited the locality, however. His opinion, founded chiefly on inspection of hand samples, was that the material was an "inspissated petroleum," or say, *a mineral pitch*. The results of my own examinations of some samples, made in January 1865, did not enable me to concur in this view, and I projected a visit to the locality, which I succeeded in carrying out in the ensuing summer. It was then accessible but by a very rough bridle path of fifteen miles, but now as I am told a branch railroad is completed from Cairo Station on the N. W. Virginia railroad.

The enclosing rocks are the ordinary blue sandstones and shales of the carboniferous, dipping generally about 15° or 17° N. W., in places nearly horizontal. The mineral occupies a vertical *dike-fissure*, it may be a shrinkage fissure, whose course is N. 76° to 80° E. A deep and narrow ravine cuts down deep into it, in the bottom of which some openings had well exposed the structure of the dike, which is obviously one of injection. My notes taken on the spot say in substance:

"The structure shows four distinct, though somewhat irregular, divisional planes, having a general parallelism with the walls. Next to the walls the structure of the mineral is coarsely granular, with an irregularly cuboidal jointed cleavage, very lustrous on the cleavage surfaces; that in immediate contact with the walls usually adhering thereto very tenaciously, as if *fused* fast to the granular sandstone.

"Next these two outside layers, which are very irregular and from two to three inches or more in thickness, is found, on each side of the vein, a layer averaging from fifteen to sixteen inches in thickness, which is composed of a variety highly columnar in structure and very lustrous in fracture, the columns being long, and, at this place, at right angles to the walls. It is this variety that was given to Professor LESLEY; as would appear from his description. Finally, in the centre of the vein, varying in thickness, but averaging about eighteen inches, is a mass differing greatly in aspect from the rest, being more compact and massive, much less lustrous in fracture, and with the columnar structure much less developed, in places not at all. The fracture and lustre of this portion of the vein are clearly *resinoid* in character.

"It is very remarkable that this curious dike-structure has heretofore escaped detection. Professor LESLEY, whose information, however, was derived at second hand, says that the mineral has 'not the slightest appearance of layers, but the aspect of complete uniformity and homogeneity.'

"The general aspect of the mass, as well as all the results of a minute examination of the accompanying phenomena, lead irresistibly to the conclusion that we have here a fissure which has been filled by an exudation, in a pasty condition, of a resinoid substance derived from, or formed by some metamorphosis of, unknown fossil matter contained in deep-seated strata intersected by the fissure or dike. It is not necessary to suppose a degree of fluidity greater than that of semi-fused pitch, or inspissated tar. Such a soft doughy mass, though flowing but slowly, would *in time* be forced by a very moderate pressure into every portion and into every crevice of the fissure. The peculiar structure described is such as would result from the fissuring of a fused or semi-fused viscous mass by the refrigeration produced by contact with the cold, and it may be wet, walls of the fissure; the outside granular layers being due to rapid cooling, and the columnar fracturing at right angles (or nearly so) to the walls (as, for example, in the case of a dike of columnar basalt) to a more gradual reduction of temperature, connected, without doubt, with the well known tendency of such materials as are susceptible of the vitreous or viscous fusion, to assume in time a concretionary or nodular structure. This tendency is strongly apparent in the brilliant variety, having produced multitudes of those curious markings on fissured surfaces which were mistaken in the case of the albertite for *fossil impressions*. The transverse columnar structure is called by LESLEY 'pencil cleavage.'

"Towards the extremities of the outcrop, where the sheet of mineral is thinner, this penicillate structure extends throughout the mass.

"The idea that this material was ever in the condition of fluid petroleum, is visionary and groundless, there being the strongest reason why it could never have been more fluid than a very thick semi-fluid pitch; of which reasons one of the most obvious is the entire absence of any penetration of the material into the surrounding porous sandstones. Also, no such substance as this, or anything approximating to it chemically, was ever known to be formed, or to have been formed, by the oxidation and inspissation of petroleum; and the formation from liquid petroleum, in such a fissure, to the depth of hundreds of feet, by any process of oxidation from the surface, of a mass so uniform as this, is an idea which I believe will receive but meagre acceptation among those chemists whose minds are free from the trammels of pet hypotheses.

"I will add, that I have yet to learn of the existence of *any* products (now forming) of oxidation or 'inspissation' (whatever that may be) of West Virginia petroleum.

"In sinking a small shaft here, twenty-eight feet deep, Mr. J. CARVILLE STOVIN, the engineer in charge at the time, found a detached fragment three and a half feet long of the north wall of the dike, imbedded in the mineral twenty-four inches distant from said wall, and twenty-nine inches vertically below the *hiatus* in the wall, marking its point of detachment; while exact measurements, both of itself and of the cavity left (on removing the mineral which occupied its original space) showed that it had become entirely inverted in position during its descent. The pitch-like semi-fluidity, which I have contended for, is here strongly illustrated, by the small depth of descent of this mass of quartzose sandstone, through a material whose density could not have been half of its own; while its distance from the wall and inverted position suggest that at the time of its detachment the dough-like mass was still rising, or in some sort of motion at least, in the dike-fissure.

"I myself observed similar *horses* of the wall rock, of small size, similarly imbedded in the mineral, at several points."

The density of a mass of the mineral was found to be 1.145. The horizontal extent of visible outcrop actually measured by me was five hundred and thirty fathoms, thinned out at east end to thirty inches, and at west end to eight inches; but as these points were at least seventy to eighty fathoms vertically higher than the bottom of the ravine, the width (averaging about fifty inches) at the latter depth points to a rapid widening of the fissure in descent. Allowing a uniform width of forty inches at the level of the bottom of the ravine, with the longitudinal extent measured, and with the above density; each fathom in depth at this level would contain two thousand tons. The depth to which it may extend is of course wholly unknown; but if the view I have provisionally adopted of the way in which this dike has originated be correct, it follows that it must widen in depth. This view I would present as follows:

By the action of heat upon strata of rock, containing fossil matter, and the pasty fusion of this fossil substance, and the partial gasefaction of itself and its contained water, with the uncontrollable expansion resulting therefrom, there has been opened a fissure through which the doughy mass, puffed up by bubbles of steam and other gases, escaped to the surface, or at least near enough to the surface to relieve the tension, and allow the steam and gases to escape gradually through the porous sandstone.

This hypothesis of mine seems the only one which reconciles an important difficulty besetting the other hypothesis (namely, that which creates a fissure originally filled with liquid petroleum, that has undergone "inspissation" from surface agencies); that is, the interference of water, which must necessarily have filled a preëxistent fissure, and opposed an obstacle to the subsequent infiltration of another fluid, at least if the latter were impelled by gravity alone. The supposition that the fissure itself was formed simultaneously with, and by, a fluid mass, containing within itself its own elastic expansive force, escapes this difficulty. I may also point out that it is not necessary to suppose that the *heat* which produced this expansive force was the central heat of the earth; for it is more probable that both the steam or gases, and the heat which expanded them, arose from a spontaneous decomposition (of the nature of fermentation) in the bed of fossil matter from which the grahamite exuded.

The view will occur that these original beds of fossil matter are beds of bituminous coal, as such coals without doubt underlie the locality. The only present support for such an idea, so far as I know, is the extraordinary statement of LESLEY, made on the oral authority of Professor HALL (see LESLEY's Manual of Coal and its Topography, p. 165), of a "leader of coal" proceeding from a coal bed downwards into a limestone quarry, and there spreading out into a layer.

Chemical and Mineralogical Characters. This mineral is, in its actual identity or individuality, not only new, but cannot even be *classed* with certainty as yet, with any other mineral substance heretofore known and investigated by chemists. It is in its behavior with solvents especially, as well as with some other chemical agents, that those peculiarities are found, which establish it as a substance *sui generis.*

To the action of acids, alkalies and oxidating agents generally, it is quite indifferent. Concentrated boiling nitric and muriatic acids, and even *aqua regia*, have no action whatever upon it; nor has boiling sulphuric acid, if somewhat diluted. Oil of vitriol, however, even in the cold, forms a brown solution. The most concentrated caustic alkaline solutions are totally without action upon it.

Alcohol does not dissolve a trace of it. Naphtha, benzole and ether dissolve part of it; oil of turpentine gradually swells it up into tar-like magma, and then dissolves most of it; but its true solvents are *chloroform* and *sulphuret of carbon*, each of which apparently dissolves nearly the whole mass of the mineral with great rapidity, leaving nothing except the mineral matter (ash, = by analysis, about two per cent.), and some small proportion of coaly matter.

When heated in the open air it endures a temperature far above that of the fusing point of asphalts (which is usually below, and never much above, the boiling point of water) without change; but when heated above 400° F. it begins to decrepitate, smoke and soften, behaving very much like a highly caking coal.

The vapor given off under these circumstances I have found to be chiefly water. There is little or no smell, such as all asphalts give out when heated, and I may remark that no odor is given out when this mineral is broken, rubbed or scraped, or even from a mass of it lying in hot sunshine. The argument deducible herefrom, that it never could have been produced by the atmospheric oxidation of any other substance, and more particularly of petroleum, will be appreciated by chemists, at least. If the heat is now raised still higher, empyreumatic vapors appear, and indications of pasty fusion in the central and lower portions of the mass, though not upon the upper surface; the fact being that under ordinary atmospheric pressure the material is incapable of fusion without decomposition; but that under a very slightly increased pressure, even such as is developed by its own pasty cohesion at the caking temperature, a pitchy semi-fusion takes place. While in this condition the resinoid (or viscous) character of the material may be made strongly apparent, for by a little dexterous manipulation these central semi-fused portions of the mass may be drawn out into long, delicate threads, like semi-fused glass, sugar, or sealing wax. To my surprise I have found that by exceedingly careful manipulation with albertite, I could thread it out in the same way, though with far greater difficulty than with grahamite.

From the above it is clear that the West Virginia mineral can neither be identified, nor even classed, with coal, asphaltum, or albertite; and I have hence thought it both admissible and just to perpetuate in connection with it the name of the gentlemen who have been so energetic in promoting a public knowledge of this new material; by conferring upon it the new name *grahamite*.

The above statements were in substance comprehended in a pamphlet printed and privately circulated in the fall of 1865.*

* An unsatisfactory account (to me) of this pamphlet was given in the *American Journal of Science*, xlii, p. 420; and DANA, in the latest edition of his Mineralogy, has adopted my name grahamite, although he still appears to take the view, which

It remains for me to present as concisely as possible, some of the results of my subsequent laboratory work upon grahamite, which, as before intimated, I am now preparing to extend farther.

The most important development made up to this time, has been the decomposition of grahamite (at least ninety-five per cent. of it) into two distinct resinoid substances, to which I have given, for reasons that will appear, the names *Viscosine* and *Irisine*. These are separated from each other by the action of solvents, viscosine being readily soluble in ether and petroleum-naphtha. The crude irisine remaining after the action of one of these solvents may then be extracted pure by one of its solvents, among which are *chloroform*, *bisulphide of carbon*, *oil of turpentine* and *benzole*. The dull compact core of the grahamite-dike contains a much smaller proportion of viscosine than the lustrous penicillate portions nearer the walls.

Ultimate organic analyses of grahamite, as a whole, have been made; but for several reasons, especially because of my observations of its mixed nature, I attach no importance to them, and shall not quote them. Analyses of pure viscosine and pure irisine, however, would be of high interest, and will in due time be made.

Properties of Viscosine. The liquid solutions formed by the action of a solvent of viscosine on the crude mineral possess a powerful olive-green fluorescence, exactly like that of many crude petroleums. The most lustrous samples of grahamite yield as much as twenty per cent. Dense solutions are as viscous as a thick syrup of glucose or a strong liquid glue.

Solid Viscosine. This is a substance of a very peculiar combination of physical and chemical characters. Though acquiring, by semi-fusion, a ductility and viscosity comparable to that of shellac and some others of the true resins, it is wholly sep-

I trusted I had overthrown, regarding the nature of the material, that it is "supposed to be like the albertite, an inspissated and oxygenated petroleum." DANA's Mineralogy, p. 753).

Since the date of the publication of the above pamphlet, a paper was presented to the American Philosophical Society (see its *Proceedings* for July 1868, p. 457), by Prof. S. F. PECKHAM, in which, without knowledge of the above, views are taken singularly approximate to those I have expressed regarding the mode of origin of minerals of this class. From recent correspondence with Prof. PECKHAM, I learn that he will soon publish a paper, in which he himself points out this coincidence.—H .W.

arated from this class of substances by its chemical properties. Thus the most concentrated alkalies, even as fused hydrates, are wholly without action on it. Sodium remains indefinitely bright in a solution of viscosine in a liquid hydrocarbon, benzole for example; whence it appears to be a *hydrocarbon*, and, I believe, the first example of a hydrocarbon possessing a resinoid viscosity. Probably its most peculiar conjunction of properties is that of its passage, within very narrow limits of temperature or permeation by a solvent, to a solid form in which it is excessively brittle and fragile. On solidification from fusion its surface is extremely brilliant. When heated, viscosine melts readily to a thin liquid and finally distils partially unchanged. In a current of superheated steam it may be distilled over without change, except a darkening in color. The crude oil obtained by distilling grahamite contains much of it, especially when superheated steam is used. Freshly obtained, by evaporation of its etherial solution, it has a distinct balsamic odor, which after some time departs. In the mass it is brown; in powder buff-color; in thin layers nearly colorless.

I believe that this viscous hydrocarbon occurs in crude petroleums, and I have obtained a product from gas-tar approaching closely to it.

Irisine is inodorous; infusible without decomposition (except, probably, under heavy pressure); black in the mass, and pure dark brown in powder. When permeated with a small quantity of a solvent it has no viscosity, but is rather gelatinous and elastic, or slightly India-rubber-like in consistence, and shrinks enormously, and cracks accordingly, in drying.

It forms in solution in turpentine, chloroform, benzole, etc., very fine lustrous varnishes, which have the extraordinary property, when somewhat thinned, of always drying on polished surfaces to a *brilliant iridescence*, the colors having a sort of metallic depth and body only to be compared with those produced by heat on polished steel, while much brighter than the latter. This property has led me to confer upon this characteristic, and main constituent of grahamite, the special name *irisine*. There is much more in it than the mere phenomenon of thin films. I should suggest an extraordinary *low* refractive index for irisine, were this not difficult to reconcile with the fact I have observed that the presence of the *minutest quantity* of most other substances (viscosine excepted) in the solution impairs or wholly destroys the colors. It seems to me rather to be intimately connected with the peculiar *pure deep brown color* of irisine (combined, it may be, with a low index of refraction), and I am strongly desirous of assistance in the matter from some gentleman experienced in optical researches. Of such I should first of all ask whether the formation, in such a thin brown transparent film, of innumerable excessively minute *fissures*, seen against a specular background, could lead to the phenomena described? I refer of course to the property of irisine, above pointed out, of shrinkage, in desiccation from the colloid condition. This hypothesis would accord with the observation of the destruction of the color by small quantities of fatty and other substances, acting by alteration of the peculiar condition of *consistence* of irisine. Experiments to test this could doubtless be devised.

Another characteristic of irisine is that it is instantaneously altered and precipitated (from its solution, as an *insoluble modification*, by mere contact with a drop of sulphuric acid, and some other agents; becoming thereby *insoluble in all menstrua* yet tried. It may thus be prepared in a state of purity for analysis; and, moreover, as *animal charcoal* is one of the agents which have this power of modifying and precipitating irisine, while it has no action (except a decolorizing one) upon solutions of viscosine, we have also here a means of obtaining the latter perfectly free from irisine for the same purpose.

To the above strongly marked peculiarities of irisine, may be added its as yet almost unique conjunction of the properties of high solubility (when in the soluble modification) in so many solvents, with high infusibility. The conjecture is offered that bituminous coals contain the insoluble modification of irisine, or of substances of its class as yet unknown. In this connection I would recall that petroleums, and even distilled coal-oils contain substances precipitated by acids, but not yet investigated.

As to the *composition* of irisine, we have as yet no analysis. Experiments with sodium, which in time precipitates it completely from its solutions, would seem to indicate an oxygenated body; but as minute quantities of alkalies, as well as acids, convert it into its insoluble form, this is inconclusive. In organic combustions, great difficulty will be found, as its coke swells up and is excessively hard of combustion. I propose introducing it, however, into the combustion tube as a chloroform solution, in a sealed bulb, which may then be broken and the solution absorbed by the oxide of copper, the chloroform being of course removed before the combustion as vapor.

The Insoluble Residue of Grahamite. This, which is very small in quantity, as left by exhaustion with chloroform, was kindly examined for me with the microscope by Dr. JOHN TORREY. He reported that there was no trace of organic structure and it appeared to be mainly quartz sand.

Comparisons with other Substances. From all such, grahamite readily separates itself. Most so-called *asphalts* (a term I should wish to confine to products of subaerial alteration of petroleums) differ from it greatly in their relations to solvents. Ether, for example, as stated by BOUSSINGAULT, DUMAS, and others, usually dissolves most of these. Still I believe that I have detected both of my new substances, viscosine and irisine, in some asphalts that I have examined.

Also, the benzole solution of albertite, comprising usually about twenty per cent. of the mineral, I find to contain viscosine, irisine, and another substance unknown as yet; eighty per cent., however, of the albertite is insoluble under ordinary circumstances, and may, possibly, though not probably, be *insoluble* irisine, or a homologue thereof. I propose farther investigation into this matter, and especially analyses of the proximate constituents of the albertite as obtained by the agencies of solvents, the only analyses, it seems to me, likely to be of much value.

BOUSSIGAULT'S *Caxitambite* comes no nearer; nor STERRY HUNT'S bituminous veins; nor "*Zopissa*" (which latter I found to be a greasy variety of asphalt).

The Colorado Dike. Four or five years since we began to hear of a curious formation in Colorado, believed to be albertite, which it very remarkably resembles in aspect. I was fortunate enough to obtain through the kindness of Dr. NEWBERRY, from the collection of the Columbia College School of Mines, a sufficient sample of an authentic specimen of the Colorado mineral to enable me to determine the surprising fact, now announced by me for the first time, that this is also *grahamite*, chemically almost identical with that of the West Virginia dike.

Its fracture is brilliant, but not so much so as albertite, and it is not penicillate like the glossy variety of grahamite. It contains scarcely any mineral impurity, dissolving wholly, without appreciable sediment, in chloroform, benzole, and oil of turpentine. Ether extracts about the same proportion as from glossy grahamite, with the same green fluorescence, and leaves, on evaporation, viscosine, identical in every way with that of grahamite. The residue from ether dissolves wholly in benzole, and gives all the reactions of irisine but is lighter in color, and the iridescences obtained are not so strong. Sulphuric acid wholly precipitates this Colorado irisine.

It may of course be anticipated that this highly characteristic species grahamite will be discovered elsewhere, two localities being already known, separated by the width of half the continent.

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4. INVESTIGATIONS OF FLAME TEMPERATURES; IN THEIR RELATIONS TO COMPOSITION AND LUMINOSITY. By BENJAMIN SILLIMAN and HENRY WURTZ.

FIRST MEMOIR.—CALORIFIC POWERS OR EFFECTS OF GASES.

THESE subjects lie, in our belief, at the very basis of the true theory of the phenomena of luminiferous gases, and have practical bearings that can scarcely be overrated.

In fact, our studies of the subject have led us in the direction of the general conclusion that, all other conditions being equal, the *temperature*, in a given flame, is the main factor of luminosity. This, however, may as yet be regarded merely as a hypothesis; in consequence of the imperfection of our present means of actual experimental demonstration of the temperatures of flames. It is a hypothesis, nevertheless, which is in general accordance with known facts. By the spectroscope, for example, which can recognize only luminous rays, we find that the higher the temperature the greater the number of these luminous rays. The recent results of FRANKLAND upon the development of luminosity by increased pressure, in flames which are non-luminous under atmospheric pressure, are in accordance with this view; increase of temperature necessarily following increase of pressure.

Very vague views have been rife, even among chemists, with regard to the temperatures of luminiferous flames. Some have been satisfied with believing crude hypotheses; such as that the heat-power of a flame is always proportional to the *density* of the gas or vapor undergoing combustion; or that it is proportional to the *amount of oxygen consumed* by a given volume of the gas; and so on. This latter hypothesis has been one of very common acceptation. A view which is even now entertained by some skilful chemists (than which, however, nothing, as will be shown below, could be more fallacious) is, that those individual gaseous compounds which impart the highest luminosity under ordinary conditions, are also the most productive of heat.

The admirable researches of the great gas-chemist, BUNSEN, of Heidelberg, placed in our possession some years ago the means of computing, at least with approximate accuracy, the heat of flames of gases of known compositions. Few, however, have properly and successfully applied BUNSEN's methods in practice. We consider it quite time that these methods should be introduced to the knowledge of Gas-Engineers, in forms available to them.

BUNSEN's formulæ for these computations are based upon the actual experimental determinations of the *total* amounts of heat developed by the combustion of different pure combustible gases with pure oxygen, made by FAVRE and SILBERMANN; and upon REGNAULT's determinations of the specific heats of gaseous products of combustion.

It is not to be maintained that FAVRE and SILBERMANN's numbers are strictly correct, but they are doubtless approximate, and at least proportionally correct among themselves.

At any rate, they are the best data we have. Those employed here are included in the following table. They are usually given in the text-books for equal *weights* of the gases, but we have reduced them to the standard of equal *volumes* also, as more suitable to our present purpose. This reduction is made simply by multiplying the equivalents for weights, by the densities as given in the third column.

TABLE I.

	Total Calorific Equivalents.		Densities on Scale of Hydrogen = 1.
	Of Equal Weights.	Of Equal Volumes.	
Hydrogen, ...	34,462° C.	34,462° C.	1.
Carbonic Oxide, ...	2,403° "	33,642° "	14..
Marsh Gas, ...	13.063° "	104,504° "	8.
Olefiant Gas, ...	11,858° "	166,012° "	14.

The meaning of this table is simply that equal weights of water would be heated by the several gases to temperatures proportional to the numbers in the first column, when equal *weights* of the gases are burned; and proportional to those in the second column, when equal *volumes* are burned.

A cursory glance at the figures in the second column of this table might seem to justify the notion hitherto entertained by many, of the comparatively low calorific powers of hydrogen and carbonic oxide, and it was doubtless as a consequence of such a comparison as this that statements have been put forth, and widely accepted among American Gas-Engineers, to the effect that the weights of water heated from the freezing to the boiling point by one cubic foot of the four main components of illuminating gas, respectively, are as follows:

Hydrogen, ...	2.22	lbs. water
Carbonic Oxide, ...	2.16	" "
Marsh Gas, ...	6.17	" "
Olefiant Gas, ...	10.74	" "

The figures here being obviously about in the same ratio as those in the second column of Table I. Several most grave errors, however, are here involved. To get at the true relative calorific effects of the above gases, when burned in the open air, in heating water below its boiling point, deductions must be made, not only for the *specific heats* of the products of combustion of the gas, but also, more important still, for the specific heat of the *nitrogen of the air* required to burn the gas. In fact, when we consider that for each volume of oxygen required to burn a given volume of gas, about *four volumes* of nitrogen must be heated up to the temperature of the flame, it becomes easy to conceive, what is actually the fact, that within certain limits the waste of heat due to this cause alone counterbalances altogether the advantage that would be supposed to result from the crowding of combustible matter into so condensed a form as in the illuminating hydrocarbons. An inevitable result of our investigations of this matter, is that the heating powers of the flames of pure hydrogen and pure olefiant gas, even when used to the greatest advantage, to heat water below its boiling point, are almost or quite identical.

In this discussion we have occasion to use the numbers representing the specific heats of but *three* gases, the three, namely, which remain after complete combustion, *steam*, *carbonic acid* and *nitrogen;* as we must assume that in the hottest and most luminous zone or shell of the flame, there is no oxygen in excess to be heated. These three numbers are, according to REGNAULT's latest determinations, for equal weights of

Steam	0.4805
Carbonic acid	0.2163
Nitrogen	0.2438
(*Liquid water* being	1.0000)

That is, the amount of heat which would raise one pound of water and steam to the same degree are in the ratio of 0.4805 for the pound of steam and 1. for the pound of water.

CALCULATION OF THE CALORIFIC EFFECT OF HYDROGEN BURNING IN AIR.

Let us take, first, the simplest case possible, that of hydrogen with exactly the right admixture of pure oxygen to burn it, which, by Table I, develops a total heat of 34462° *C.;* that is, would heat a certain weight of *liquid* water to this temperature. In order to find the actual amount of heat contained in the products of combustion, we must first take into account the fact that one pound of hydrogen burns to *nine pounds* of steam, and then obtain the ratio between the above number, 34,462, and the amount of heat necessary to heat nine times the weight of steam, that is nine times the specific heat of steam. Calling the total residual heat in the produced steam x, we have the simple proportion:

$$9 \times (\text{Sp. heat of Steam} = 0.4805) : 34462 :: (\text{Sp. heat of Water} = 1) : x;$$

$$\text{or, } x = \frac{34462^\circ}{4.3245} = 7969^\circ\ C.^* = 14376^\circ\ F.;$$

a number which, we may add, represents the *maximum* of heat capable of being imparted *to liquid water* by the flame of HARE's oxyhydrogen blow-pipe.

Still, we have by no means here the actual temperature of the free or open flame of HARE's blow-pipe; which is greatly *lower* than this figure; as we have not yet taken into account the "latent heat," or heat of vaporization, of the nine pounds of steam formed. The Centigrade temperature necessary to convert one pound of water into steam being 537°; to get the *actual temperature* of the oxyhydrogen flame, we must modify the above equation, so that

$$x = \frac{34462^\circ - (9 \times 537^\circ)}{4.3245} = 6851^\circ\ C. = 12364^\circ\ F.$$

which is the temperature actually possible in the flame of the compound blow-pipe, *were the combustion instantaneous and complete.*

When hydrogen gas burns *in air*, however, as has been before stated, another deduction of enormous amount must be made from the above figures, due to the heat required to expand the nitrogen. This is obtained simply by adding to the divisor, as above, the weight of the nitrogen of the air employed, multiplied by its specific heat. The weight of the

* BUNSEN, in his Gasometry (English edition of 1857, p. 242), gives this number as 8061° *C.*, the difference being due to the use by him of a different number for the specific heat of steam, namely, 0.475, apparently an earlier determination of REGNAULT. BUNSEN makes here the singular oversight of regarding this figure as the temperature, when "the gases can freely expand, as is the case in an open flame," overlooking the correction necessary in this case for the *latent heat of steam of combustion*, as is explained in the text above.

nitrogen in air = 3.318 times the oxygen; so that the latter of the above equations becomes

$$x = \frac{34462° - (9 \times 537°)}{4.3245 + (8 \times 3.318 \times 0.2438)} = 2744.5° \ C. = 4972° \ F.$$

[We wish to point out that we have here a full explanation of the extraordinary rate of degradation of illuminating gas by admixture of air, which we have discussed elsewhere. The nitrogen of such air is not merely a diluent, or even a mere deductive quantity; its specific heat is an actual *divisory* function in diminishing the flame temperature.]

This, then, is the actual temperature which the flame of hydrogen gas burning in the atmosphere might attain to, supposing complete and instantaneous combustion. If it is desired to obtain instead, the total calorific effectiveness, as in heating water below its boiling point—in which case the latent heat of the steam of combustion becomes also available—the above expression is changed by simply omitting the subtrahend in the numerator:

$$x = \frac{34462°}{4.3245 + 6.4714} = 3192° \ C. = 5778° \ F.$$

Calculation of the Calorific Effect of Carbonic Oxide Burning in Air.

As the product of combustion is here solely carbonic acid, no latent heat of steam enters, and the calorific effectiveness is the same, under all circumstances, in air. In the numerator we substitute of course the calorific equivalent of one volume of carbonic oxide from Table I; and in the denominator, for the specific heat of nine pounds of water, that of twenty-two pounds of carbonic acid, being the weight of the latter formed by the combustion and combination of fourteen pounds (weight of a volume of carbonic oxide on the hydrogen scale by third column of Table I.) of carbonic oxide, with eight pounds of oxygen. The number for the specific heat of nitrogen is the same as before, and the equation is now

$$x = \frac{33642°}{(22 \times 02168) + 6.47 = 11.23} = 2996° \ C. = 5425° \ F.$$

Marsh Gas and Olefiant Gas.

In these two cases, we have as products of combustion both carbonic acid and water; and, therefore, when the calorific effects are sought for, we have not only the latent heat of steam entering as a subtrahend into the numerator; but also into the denominator, as divisors, all three of the specific heats of steam, carbonic acid and nitrogen.

Then, as eight pounds of marsh gas consume thirty-two pounds of oxygen, and produce twenty-two pounds of carbonic acid, and eighteen pounds of steam; and as fourteen pounds of olefiant gas consume forty-eight pounds of oxygen, producing forty-four pounds of carbonic acid, and eighteen pounds of steam, the equations for the calorific powers of their flames in air become—

For marsh gas:

$$x = \frac{104504^\circ - (18. \times 537^\circ)}{(18. \times .4805) + (22. \times .2163) + (32. \times 3.318 \times .2438)} = 2414^\circ \text{ C} = 4386^\circ \text{ F.}$$

And for olefiant gas:

$$x = \frac{166012^\circ - (18. \times 537^\circ)}{(18. \times .4805) + (44. \times .2163) + (48. \times 3.318 \times .2438)} = 2743 \text{ C.} = 4970^\circ \text{ F.}$$

The following Table gives all the results of our calculations by the above methods.

TABLE II.

For Equal Volumes of the Gases Burning in Air.	Calorific Effects in Heating Liquid Water.		Calorific Effects above 100° C.	
	Centigrade Degrees.	Fahrenheit Degrees.	Centigrade Degrees.	Fahrenheit Degrees.
Hydrogen (Sp. Heat HO = .4805)	3192°	5778°	2744°	4971°
Hydrogen (Sp. Heat HO = .4750)	3204°	5799°	2755°	4991°
Hydrogen (Mean, ...	3198°	5788°	2749°	4980°
Carbonic Oxide, ...	2996°	5425°	2996°	5425°
Marsh Gas, (Sp. Heat HO = .4805)	2660°	4820°	2414°	4386°
Olefiant Gas, " " .4805)	2916°	5481°	2743°	4970°

COMPUTATIONS OF CALORIFIC EFFECTS OF MIXED GASES.

The above Table renders the calculation of the calorific effects of any given gaseous mixture, whose centesimal composition is known, a matter of extreme simplicity. It is only necessary to obtain the sum of the multiples of the percentage of each component gas into its calorific capacity, as given in this Table, and divide by 100.

To serve as examples of these modes of computation, we here cite, in tabular forms, the results of some analyses of a number of gaseous mixtures made by us during the past winter (1868-69). [These analytical results, it may be remarked, possess points of novelty and importance, both scientific and practical, which will bring them up again hereafter, in other connections. They are here placed on record.]

Table III, gives the results of two analyses of gaseous mixtures obtained by passing steam *superheated to incandescence* upwards through a mass of *anthracite coal* heated to a high degree in a clay retort of a novel construction, according to what is now known as the *Gwynne-Harris*, or *American Hydrocarbon-Gas System*. In this table the results are calculated without carbonic acid and sulphuretted hydrogen, which, with traces of nitrogen, and sometimes of oxygen, are found in the unpurified anthracite gas.

TABLE III.

ANALYSES OF ANTHRACITE HYDROCARBON GAS:

BY SILLIMAN AND WURTZ.

	1.	2.	Mean.
Hydrogen, ...	60.43	59.32	59.87
Carbonic Oxide, ...	35.44	37.14	36.29
Marsh Gas, ...	4.13	3.54	3.84
	100.00	100.00	100.00

In Table IV, column (1) gives the results of an analysis of the street gas served out at this period by the *New Haven Gas-Light Co.*, made from Westmoreland coal enriched with about six per cent. of albertite. Column (2) exhibits the mean of four analyses of the completed Hydrocarbon Gas, made by us at Fair Haven during the same time by combining gas from the same Westmoreland coal (with ten per cent. of albertite) with half its volume of the anthracite gas.

Columns (3) and (4) are obtained from (1) and (2), by centesimal reduction, after deduction of the illuminant ingredients, being what we propose to designate as the non-illuminating *substrata* of illuminating gases.

TABLE IV.

ANALYSES OF ILLUMINATING GASES: BY SILLIMAN AND WURTZ.

	(1.) New Haven City Gas.	(2.) Fair Haven Hydrocarbon Gas.	(3.) *Substratum* of New Haven Gas.	(4.) *Substratum* of Fair Haven Gas.
Hydrogen. . .	43.58	46.77	46.79	50.27
Carbonic Oxide,	2.14	9.56	2.31	10.27
Marsh Gas, . .	47.42	36.71	50.90	39.46
Illuminants, . .	6.86	6.96		
	100.00	100.00	100.00	100.00

Table V. gives the results of the computations, from our *formulæ*, of the calorific powers of these five gaseous mixtures, for communicating temperatures both above and below that of aqueous ebullition. We should remark that we have here been obliged to regard the volumes of *illuminant hydrocarbons* as representing olefiant gas solely; both because we have no certain data as to their real nature, and particularly because, if we actually knew, or should assume, the nature of the hydrocarbon vapors present, still we have no experimental calorific equivalents, as we have for olefiant gas, from which to start, in such a computation. We have reason to believe that the errors thus introduced are not important in amount.

The last two columns of Table V. have been calculated to furnish a direct comparison, for each of these gases, of its calorific power compared with that of the New Haven street gas, the latter taken as = 100.

TABLE V.

CALORIFIC EFFECTIVENESS OF GASEOUS MIXTURES; COMPUTED FROM THE FORMULÆ AND ANALYSES OF

SILLIMAN AND WURTZ.

	Weights of Water Equally Heated *Below* 100° C. by Equal Volumes.	Weights of Water Equally Heated *Above* 100° C. by Equal Volumes.	First Column reduced to New Haven Gas = 100.	Second Column reduced to New Haven Gas = 100.
Anthracite Gas, . .	3100	2823	104.2	109.2
New Haven Gas; *Substratum*,	2917	2581	98.1	99.6
Fair Haven Gas; *Substratum*,	2962	2640	99.6	102.0
New Haven Gas; with Illuminants assumed = Olefiant,	2974	2593	100.0	100.0
Fair Haven Gas; with Illuminants assumed = Olefiant,	2959	2647	99.5	102.1

Conclusions.—Some of the practical conclusions to which we are of necessity compelled, by the results of the above investigations, are so novel and remarkable, that we feel diffident regarding them. It is, however, always safe to follow the leading of Truth, however astray she may lead us from our preconceived notions.

It is apparent from Table II:

1. That of all known gases, the highest calorific effects, under ordinary atmospheric conditions, are obtainable from *carbonic oxide;* whose calorific value, above 100° *C.*, is about 3000° *C.*

2. That in absolute calorific value, below 100° *C.*, in the atmospheric medium, hydrogen surpasses its volume of any other gas; giving a temperature of about 3,200° *C.*

3. That for all modes of application—that is, for producing both high and low temperatures—the total maximum calorific effectiveness of carbonic oxide is a *constant quantity.**

* Metallurgists, especially, will appreciate the suggestive import of the truths presented under the first and third heads; here enunciated, as we think, for the first time. It is to be noted that all the above effects belong to the *maximum* kind, and, of course, reach their development only under the most favorable conditions in each case respectively.

4. Compound condensed submultiple volumes of hydrogen, like that in marsh gas, have much *less* total calorific value in air than their volume of free hydrogen.

5. Condensed compound submultiple volumes of gaseous carbon, like that in olefiant gas, have no greater total calorific value, in air, below 100° *C.*, than their own volume of carbon gas in the form of carbonic oxide; while above 100° *C.*, their value is even considerably less.

The above investigations of calorific powers has been found by us a necessary preliminary to a more important and far more difficult experimental investigation of the subject of *Flame Temperatures*, which we have in progress, and upon the condition of which we hope to report at an early day. As it is fully recognized now (from the results of BUNSEN, BERTHELOT, DEVILLE and DEBRAY, and others) that in no body of burning gases, at any one time or place, does the *entirety* of the combustible constituents enter at once into the combustion; it is clear that the actual maximum temperatures, or calorific *intensities* of the flames of the above gases can in no case be so high as the figures established as above, by us, for their total calorific *powers*. Conditions enter here which render this problem one of the most complex and difficult that has yet been attacked by chemists.

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5. ON SOME NEW PROPERTIES OF PHOSPHORIC ACID. By E. N. HORSFORD of Cambridge, Mass.

IT has long been known that phosphoric acid exists in three well marked modifications, distinguished as

Ordinary Phosphoric Acid,
Pyrophosphoric Acid, and
Metaphosphoric Acid.

The first combines with three atoms of base, the second with two atoms, and the third with but one. To Dr. Clarke of Aberdeen we owe the discovery of pyrophosphoric acid, and to Prof. Graham, of the Royal Mint, the discovery of metaphosphoric acid and the clearer definition of the characteristics by which the three acids are distinguished from each other. To Fleitmann, Henneberg and Maddrel are we indebted for a large addition to our knowledge of phosphoric acid, and especially of metaphosphoric acid. These chemists have presented us a series of metaphosphoric acids, which in their salts may be represented

$$MeO. \quad Po_5$$
$$2\,MeO. \quad 2\,Po_5$$
$$3\,MeO. \quad 3\,Po_5$$
$$4\,MeO. \quad 4\,Po_5$$
$$6\,MeO. \quad 6\,Po_5$$

and an acid of the constitution $6\,MeO. \quad 5\,Po_5.$

Dr. Clark long ago pointed out that ordinary phosphoric acid in solution concentrated till its temperature rose to 215° *C* was converted for the most part into pyrophosphoric acid. The same acid may be produced by ignition of tribasic phosphate of soda, and the metaphosphate by ignition of microcosmic salt — tribasic phosphate of soda, ammonia and water.

In these latter cases of conversion by heat, the characteristic acid was determined by the measure of fixed base present.

To these laws I have to add two more:

First, That whatever the relation of fixed base to acid is between one and three of base to one of acid, on ignition the acid potentiality subsides precisely to the horizon of the fixed base present.

Second, That within certain limits, greater, however, than these, that is, containing less than one of base to one of acid, this metamorphosis may take place when the base is lime by simple drying at ordinary temperatures; thus, for example, one of base to one of tribasic phosphoric acid, allowed spontaneously to dry by evaporation, will become the uniting equivalent of the base present — that is, the salt will become inert.

These two laws I discovered several years since, and desire to place them on record, as a duty to myself, although I am not prepared to present the full evidence which justifies the announcement. I may state, however, as a general illustration, that I have prepared an acid phosphate in crystalline form, in which the acid exceeded the ratio of one of tribasic acid to one of base, and have observed that whereas it had at the time of its preparation an acid strength which I will indicate by 100. A few days later its acid strength has fallen to 90; later still to 80; later still to 70; later still to 50; a year later to 0.

This acid, however, in contact with water, after a long time resumed its full energy, and in contact with a solution of soda resumed it with much greater rapidity.

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6. Phosphoric Acid, Iron and Potassium, Constituents of Chlorophyl. By E. N. Horsford of Cambridge, Mass.

At the meeting of the Association at Chicago, I announced the discovery of phosphoric acid as a constituent of the etherial solution of butter.

As the butter is derived from the food which the cows receive, the suggestion that it might be found in the ethereal solution of grasses and clover was natural. These were treated with ether, the solution, after two or three days, poured off and spontaneously evaporated to dryness. The residue was ground intimately with several times its volume of magnesia, previously demonstrated to be free from phosphoric acid; the mixture burned to whiteness and tested with nitric acid and molybdate of ammonia. This gave at once the characteristic yellow precipitate of phospho-molybdate of ammonia, thus proving the presence of phosphoric acid in the chlorophyl. The phosphoric acid was also determined as phosphate of ammonia and magnesia.

The chlorophyl was also treated directly with nitric acid, and where the ether had evaporated, and the wax-like body separated, to float on the surface of the liquid. Molybdate of ammonia gave from the solution the yellow precipitate, indicating the presence of phosphoric acid.

The readiest method for recognizing the phosphoric acid is to agitate the ethereal solution of chlorophyl with one-fourth its volume of concentrated muriatic acid, which resolves the green into two layers, one the blue below, the other the yellow above, as observed by Payen; and then separate the blue by dialysis into water containing a trace of ammonia. This liquid on the addition of nitric acid and boiling for a moment, gives, with molybdate of ammonia, the characteristic reaction for phosphoric acid. With sulphate of magnesia and ammonia, it gives the characteristic precipitate of ammonia-phosphate of magnesia.

From the same solution sulpho-cyanide of potassium gave the characteristic reaction for iron; and fero-cyanide of potassium gave with proper dilution at first only a green shade, becoming after exposure to the air for some time, Prussian blue, thus confirming the discovery of Verdeil of the presence of iron in chlorophyl. The flame of the chlorophyl residue gave with the spectroscope the red and blue lines of potassium. This observation, conducted with great care with the aid of Prof. Winlock and Prof. Watson, was so many times repeated as to leave no room for doubt on the point.

The examination for phosphoric acid was continued in the chlorophyl of the leaves of Indian corn, of the potato, of melons, squashes, sunflowers, the pear, cherry, apple, the Virginia creeper, the westeria and bignonia, the ash, elm, maple, oak, sumach, many varieties of shrubbery and flowering plants, the arbor vitæ, numerous fresh and salt water plants and indeed of most of the varieties of green vegetation that were accessible. Phosphoric acid was found in all. The ethereal extract of the larvæ feeding on the elm, which was for the most part chlorophyl, contained phosphoric acid.

It was noticed that the chlorophyl extract, when spontaneously dried down gave in each case some shade distinguished on account of its brightness, or dullness, or perishability, when kept for a short time.

In the ethereal extract of the colored petals of several cultivated flowers, I found no phosphoric acid. It was wanting in the blue blossom of cichory. In the red flower of the bignonia it was found, but there seemed to be a certain quantity, small, but actual, of chlorophyl present. It was found only in traces in the extract of the purely red portion of the leaves of the coleus and zinnæ. In the extract of the red leaves of ripening Indian corn it was less and less, or wholly wanting, and the same was true of the yellow autumn leaves of the oak, maple, sumach and numerous other colored leaves which were examined.

While the green potato leaves yielded phosphoric acid in the ethereal extract, the white sprouts of potatoes grown in the dark did not contain it.

A great variety of fungi (mushrooms, edible or otherwise), of white, red and yellow coloration beneath, for the most part yielded to ether an extract containing waxy or fatty bodies, in which phosphoric acid was found.

The ethereal extract of English breakfast tea gave phosphoric acid, as did that of the wax of the bayberry, while that of bees' wax did not. It was not found in the ethereal extracts of white or brown muscovado or cane sugar, or cod liver oil. It cannot be doubted that phosphoric acid, iron and potassium are constituents of chlorophyl.

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7. On the Relation Between the Intensity of Light Produced from the Combustion of Illuminating Gas and the Volume of Gas Consumed. By Benjamin Silliman of New Haven, Conn.

In photometric observations made to determine the illuminating power or intensity of street gas, it is the practice of observers to compute their observations upon the assumed standard of five cubic feet of gas consumed for one hour, and in the constantly occurring case, of a variation from this standard, whether in the volume of the gas consumed or in the weight of spermaceti burned, the observed data are computed by the "rule of three," up or down, to the stated terms. The standard spermaceti candle is assumed to consume 120 grains of sperm in one hour, a rate which is rarely found exactly in actual experience.

For example, a given gas, too rich to burn in a standard Argand burner at the rate of five cubic feet per hour without smoking, is consumed at the rate of 3½ cubic feet to the hour, with an observed effect of 20 candles power. This result, previously corrected by the same rule for the sperm consumed, is then brought to the standard of five cubic feet by the ratio $3.5 : 20 = 5 : 28.57$.

The candle power of the gas is therefore stated as 28.57 candles, and this result has been universally accepted as a true expression of the intensity of the gas in question, or the relative value of the two consumptions.

In common with other observers I have long suspected that this mode of computation was seriously in error, as an expression of the true intensity of illuminating flames, and that there were other conditions besides the volume of gas or weight of sperm consumed which must influence, and greatly modify the results. As most of these conditions are considered somewhat at length in a paper on *Flame Temperatures*, prepared chiefly from researches conducted by Prof. Wurtz and myself, and presented at the Salem Meeting of the Association, they need not be discussed in this connection.

The results of many trials, made with the purpose of determining the value of these photometric ratios, indicate clearly, that the true ratio of increase in intensity in illuminating flames is, within certain limits, expressed by the following theorem, viz.:

The intensity of gas flames, i. e. *illuminating power*, *increases* (within the ordinary limits of consumption) *as the square of the volume of the gas consumed.*

As the first experimental demonstration of this theorem was made by Mr. William Farmer, the photometric observer at the Manhattan Gas Co.'s works in New York, I propose to speak of it as "Farmer's theorem." I am also indebted to Mr. Farmer and to Mr. Sabbaton, the well known and courteous Engineer of the Manhattan Gas Light Company, for the free use of their experimental data, and the permission to employ them in illustration of Farmer's theorem.

The fundamental importance of this new mode of computation will at once appear, if assuming it, for the sake of illustration to be true, we apply it to the case already given above which then becomes:

$$3.5_2 : 20 = 5_2 : 40.$$

Showing an increase of about forty per cent. over the old rule of correction. Let me see how far this theorem is sustained by the test of experiment.

Experiment 1*st*. Two similar gas flames, one at each end of the photometer bar, were made to give exactly the same intensity of illumination. This was accomplished of course by placing the Bunsen disc midway between the two burners, and regulating the combustion until the disc was perfectly neutral; the consumption being noted equal by two wet gas meters under the same pressure. The screen was then moved upon the bar to a point just four times as far from one flame as it was from the other, *i. e.*, the bar being 100 inches the screen stood at 80, *i. e.*, as 1 : 4. The light from the distant burner was then increased until the disc again showed an equality of illumination. On reading the rate of gas consumed by the two burners respectively, one gave 3.66 cubic feet and the other 7.32 cubic feet, or exactly double, or in other words the lights were as the square of the volumes of gas consumed, thus: $3.66_2 : 7.32_2 = 1 : 4$.

By the old rule the intensity would have been estimated directly as the volume of the gas consumed, thus:

$$3.06 : 7.32 = 1 : 2.$$

Experiment 2*d*. The following results were obtained with the use of a standard Argand burner. The readings of the index meter, the gas consumed in cubic feet, and the ratio of the lights produced, are given in three columns, viz.:

Index,	.0550	=	3.30 feet.	=	1	light.
"	.0725	=	4.35 "	=	2.1	"
"	.0856	=	5.18 "	=	3.2	"
"	.0926	=	5.55 "	=	4.	"

In this series the lights increase in considerably higher ratio than is required by Farmer's theorem, which demands 6.60 cubic feet corresponding to a fourfold consumption, while the actual consumption was 1.05 cubic feet less than the quantity required by the theorem.

Experiment 3*d*. The following series was obtained by another Argand burner.

Index,	.062	=	3.72 cub. feet,	=	1	light.
"	.0814	=	4.88 "	=	2	"
"	.1000	=	6.00 "	=	3	"
"	.1203	=	7.219 "	=	4	"

In this series the ratio is more nearly in accordance with the demands of the theorem, the intensity being still a little in excess of the squares of consumption ($3.72 \times 2 = 7.44$ in place of 7.219).

The gas employed in these comparisons had a candle power of about 14 candles.

Experiment 4th. Results obtained by a comparison of fish-tail burners, ratio as 4 and 9 feet respectively.

A.	Index,	.0750	=	4.500	cub. feet.	=	1	light.
B.	"	.1586	=	9.519	"	=	4	"

In this comparison the ratio falls but little short of the demands of the theorem.

Experiment 5th. Comparison of fish-tail burners.

A.	Index,	.086	=	5.16 feet.	=	1.85.
B.	"	.1677	=	10.06 "	=	4.

In this trial the departure from the requirements of the theorem is considerably greater than in any of the preceding experiments. But it appears that from some cause the ratio of the squares does not hold with gas of the power used in these trials (14 candles), where the consumption rises above 9 or falls much below 3 cubic feet. This is undoubtedly connected with the well recognized fact, that there is for each gas a kind of burner and a volume of gas better calculated than any other, to develop its maximum intensity.

Experiment 6th. This series was designed by Mr. Farmer to test by a direct comparison the value of the new as contrasted with the old method of correction. Both trials were made upon the same gas, the second observation following immediately after the first and with the same candle, and therefore should give about the same candle power.

1*st Trial.* Consumption of sperm	32.7	grains.
" of gas	5.004	cubic feet.
Mean candle power (15 observations)	13.93	candles.
2*d Trial.* Consumption of sperm	32.2	grains.
" of gas	4.58	cubic feet.
Mean candle power of 15 observations	11.8	candles.

The above data calculated by Farmer's theorem.

5.004	cubic feet, and	32.7 grains give	15.15	candles.
4.58	" " "	32.2 " "	15.09	"
		Difference,	.06	"

Calculated by the old rule.

5.004	cubic feet and	32.7	grains	give	15.16	candles.
4.58	" "	"	32.2	"	"	13.82	"
			Difference,			1.34	"

It is obvious from the study of these results, that within the limits named the increase of intensity in gas flames, whether naked or Argand, is at a ratio certainly as great as the squares of the volumes of gas consumed; and hence it follows that all the photometric determinations, which have been obtained by computation from volumes greater or less than the assumed standard of five cubic feet per hour, in the simple ratio of the volumes consumed must be considered as quite worthless, provided the theorem of Farmer here announced is confirmed.

It is evident also that this theorem applies with equal force to the weight of sperm consumed by the standard candle as to the volumes of the gas burned in equal times.

With a view to test the theoreom of Farmer, I at once sought to apply it to the case of certain observations, I had made upon very rich gas obtained from cannel and other rich coals. The photometric power of these gases had been measured in the usual way heretofore practiced by observers, by burning a less quantity than five cubic feet in the standard Argand and then computing up to a standard of five cubic feet by direct ratio. The results of this comparison appear to go far to confirm Farmer's theorem.

Peytona Gas. This gas was made from a coal of West Virginia, known as Peytona Cannel Coal. It was much too rich to permit the flow of five cubic feet from the 15 hole Argand burner, with a perfect combustion. The gas was therefore reduced by mixture with a measured volume of street gas of known value and the illuminating power of the mixture having been carefully determined the value of the Peytona gas alone was readily calculated and fixed at 42.79 candles. The following trials exhibit the result obtained by burning smaller volumes of Peytona gas, and the values obtained by the two methods of calculation.

No. 1	Argand	burner	consuming	5	cubic feet	per hour, mixed gas =	42.79 candles.
2	"	"	"	3.24	"	"	18.95 "
3	"	"	"	3.48	"	"	20.94 "

Here No. 1 represents very nearly the true illuminating power of the gas, and may be assumed as a fair criterion of the law under consideration.

By Farmer's Theorem.

No. 2 becomes $3.24^2 : 18.95 = 5^2 : 45.12$ candles.
3 " $3.48^2 : 20.94 = 5^2 : 43.22$ "

By direct ratio (old rule).

No. 2 becomes $3.24 : 18.95 = 5 : 29.24$ candles.
3 " $3.48 : 20.94 = 5 : 30.09$ "

By this it appears that by the old rule, assuming the true candle power of the gas to be 42.79 candles, the two observations, Nos. 2 and 3, are in error by about 30 per cent., while by Farmer's theorem the error is reduced to 3 per cent, the former being too small and the latter too large.

Albert Gas. The well known *Albertite* of New Brunswick, furnishes a gas of remarkable richness. Its true candle power can be measured only by diluting it largely with street gas of known value, and calculating it from the determined intensity of the mixture. In this way the gas from Albertite is shown to have an intensity equal to 70.38 candles. The following results were obtained by consuming different volumes in the burners named.

No. 1	Argand burner	consuming	5	cubic feet	= 70.38 candles.
2	"	"	2.25	"	= 16.39 "
3	'Scotch tip' "	"	3.	"	= 25.25 "

By Farmer's Theorem.

No. 2 becomes $2.5^2 : 16.39 = 5^2 : 65.56$ candles.
3 " $3^2 : 25.25 = 5^2 : 70.14$ "

By simple ratio.

No. 2 becomes $2.5 : 16.39 = 5 : 32.78$ candles.
3 " $3 : 25.25 = 5 : 42.08$ "

The differences from the assumed standard of 70.38 candles are as follows:

By the old rule,	No. 2	falls short	37.6	candles or	115 pr. ct.
" Farmer's theorem	"	" "	4.72	"	7.1 "
" the old rule,	No. 3	" "	28.30	"	67.25 "
" Farmer's theorem	"	" "	0.24	"	0.34 "

It will be observed that No. 2, in this series, represents a consumption considerably below the minimum which in most cases experiment has shown to be the limit of the proposed theorem, namely: 3 cubic feet, while No. 3, which represents exactly this limit, brings the result within the range of experimental error — it being impossible to make two series of 15 photometric observations which will accord more closely than these.

Gas from the Ritchie Mineral. This gas was made from the mineral called *Grahamite* by Prof. Wurtz, and found in Western Virginia.

The gas made from this mineral was too rich to burn in the Argand burner with 5 cubic feet per hour.

1st. A mixture with Argand burner, and 5 cubic feet consumption.

80	per cent.	street gas	= 15.11	candles.
20	" "	Ritchie "	= 31.76	"
100	" "	Mixture	= 18.44	"

$$\frac{18.44 - 15.11 \times 80}{20} + 18.44 = 31.14 \text{ candles.}$$

2d. A mixture with Argand burner, and five cubic feet consumption.

78	per cent.	poor gas	= 13.72	candles
22	" "	Ritchie	= 31.01	"
100		Mixture	= 17.53	"

$$\frac{17.532 - 13.720 \times 78}{22} + 17.532 = 31.05. \text{ candles.}$$

3d. Ritchie gas burnt with Scotch tip, and 5 cubic feet consumption, gave 30.01 candles.

4th. Ritchie gas, with Argand burner, and 4.056 cubic feet consumption, gave 19.24 candles, and for 5 cubic feet consumption, by squaring, 29.32 candles, and by old rule, 23.71.

No doubt the consumption here was too much for the Argand burner, as the illuminating power falls off somewhat from the preceding trials.

5th. Ritchie gas with Argand burner, and 3.876 cubic feet consumption, gave 18.66 candles; and for 5 cubic feet consumption by squaring 31.05 candles; and by the old rule, 24.07 candles.

6th. Ritchie gas with Argand burner, and 3.828 cubic feet consumption, gave 1879 candles; and for 5 cubic feet consumption by squaring 32.05 candles; and by old rule, 24.54 candles.

Mean of the 1st and 2nd tests by mixing	= 31.40 candles.		
" " " 4th, 5th and 6th tests by squaring	= 30.80	"	difference .60.
" " " 4th, 5th and 6th tests by old rule	= 24.10	"	" 7.30.

These results show plainly that the new theorem brings all the tests very near to the true illuminating power; certainly much nearer than the old rule.

Wollongong Gas. This gas was obtained from *wallongonite*, a new carbohydrogen described by me in the number of Silliman's Journal (for July, 1869), as coming from Australia. Its illuminating power was determined by mixing 10 per cent. of the gas with 90 per cent of street gas. But this mixture was still too rich to burn 5 cubic feet in the Argand standard without smoking, and even when burned at this rate in a fish-tail burner the flame was somewhat smoky and inclined to "tail off." I have, therefore, little doubt that its true candle power is more nearly 142 candles, than to 132, as stated in the article referred to. We quote, however, the observations made, as follows:

1	fish-tail burner	consuming	5	cubic feet	gave	132.94	candle	power.
2	"	"	1.5	"	"	12.89	"	"

Computing the second observation we have:

By Farmer's theorem	for	No. 2,	143.22	candle	power.
" direct ratio	"	"	42.96	"	"

This is an extreme case in which the volume of gas consumed in the second observation is far too low, but it is clear that by the old rule the result coming from the consumption of so small a volume of gas is perfectly worthless, while by Farmer's theorem the difference of 10.28 candles is within 7.7 per cent., and if the true intensity of this remarkable gas is placed, as there is good reason to believe it should be, at 142 candles, the agreement in the two observations is absolute.

Every photometric observer can confirm the results here given by reference to his own records of former observations, or by direct experiment designed to test the accuracy of the theorem now announced.

In Sugg's *Gas Manipulation* (London, 1867), p. 64, will be found the following results from an experiment designed to illustrate the unfitness of the rule-of-three for calculating the illuminating power of gas, when using any other consumption than 5 cubic feet per hour.

Argand Burner.

1.	5.	cubic	feet	=	14.00	candles.		Differences.		
2.	4.9	"	"	=	13.78	"	—	0.22	of a	candle.
3.	4.8	"	"	=	13.74	"	—	0.26	"	"
4.	4.7	"	"	=	13.30	"	—	0.70	"	"
5.	4.6	"	"	=	13.04	"	—	0.96	"	"
6.	4.5	"	"	=	11.93	"	—	2.07	"	"

The foregoing demonstrates two facts; first, that the Birmingham burner, even when burning the full quantity of 5 cubic feet per hour, does not show the true quality of the gas; and secondly, the inapplicability of the rule-of-three for estimating the light given in proportion to the quantity consumed, otherwise the whole of the results would have been 14.00 candles."

The quality of this gas, as exhibited by Dr. Letheby's burner, with 5 cubic feet consumption, and 120 grains of sperm per hour, was 15.50 candles.

The mean candle power of the five last tests is 13.158; diffence, 0.84 parts of a candle.

The mean candle power of the above tests, before they were corrected for five cubic feet consumption, will be as follows:

1.	5.	cubic	feet	=	14.00	candles.
2.	4.9	"	"	=	13.504	"
3.	4.8	"	"	=	13.190	"
4.	4.7	"	"	=	12.502	"
5.	4.6	"	"	=	11.996	"
6.	4.5	"	"	=	10.738	"

The above corrected for five feet consumption by Farmer's theorem.

1.	5.	cubic	feet	=	14.00	candles.
2.	4.9	"	"	=	14.060	"
3.	4.8	"	"	=	14.312	"
4.	4.7	"	"	=	14.148	"
5.	4.6	"	"	=	14.191	"
6.	4.	"	"	=	13.255	"

The mean candle power of the 5 last tests is 13.99; difference 0.01 parts of a candle.

The following will show the fractional power required to bring the five last tests to 14.00 candle.

2. $4.9^{1.88}$: 13.504 :: $5^{1.88}$: 14.00 candles.
3. $4.8^{1.47}$: 13.190 :: $5^{1.47}$: 14.00 "
4. $4.7^{1.85}$: 12.502 :: $5^{1.85}$: 14.00 "
5. $4.6^{1.86}$: 11.996 :: $5^{1.86}$: 14.00 "
6. $4.5^{2.52}$: 10.737 :: $5^{2.52}$: 14.00 "

The above shows that the 3rd and 6th tests have not been good ones, or why should they differ so much from the one preceding them, the difference in consumption being only one-tenth of a foot?

The 2d, 3d, 4th and 5th tests fall a little below the square or 2d power, and the 6th test is considerably more.

The fractional powers of the 2nd, 4th and 5th are nearly the same.

I have endeavored to apply this theorem to some of the results recorded in the well known researches of Messrs. Audouin and Bérard,* but I find these results stated in a manner which renders it difficult to fix clearly the terms of comparison. I venture, however, to append a few comparisons drawn from two of the tabular records of experiments with butterfly or bats wing burners of the "fifth series" which, so far as they go, lend confirmation to the views here presented.

Consumption of the Burners under trial.	Consumption of the Bengel Argand standard burner without cone, 8 inch chimney.	Comparative intensities. The Bengel burner = 100.	Intensities by law of the squares of consumption.	Pressures.
Cubic feet.	Cubic feet.			
3.1079	3.6024	50	103	.23622
2.4015	3.5318	40	90.9	.19685
2.0131	3.6024	30	96.	.11811
Burners of same series—slit $\frac{1}{63}$ inch wide.				
3.9555	3.6730	80	92.6	.078474
3.1786	3.6730	60	80.7	.07480
2.6487	3.6730	50	96.7	.07480
2.3309	3.6730	40	97.5	.03937
1.5186	3.6730	20	115.6	.01968

* *Ann. de Ch. Et Phys.*, vol. lxv, p. 423, 1862.

The comparison of their results by this theorem, which gives reasonably exact results for consumptions which are not greater than that of the standard Bengel burner employed by them, fails when the consumption becomes greater than that of the standard.

A comparison of the foregoing results will show that the coincidences with the requirements of the theorem of Farmer are, within the limits assigned, too numerous, and too closely accordant, to be considered as otherwise than pointing clearly to its general truth. A rigorous demonstration cannot be expected as there are too many variable functions of unknown value involved in the best methods at present known for photometric measurements to permit more than an approximate proof of its general accuracy. Every photometric observer must recognize its importance and the necessity in his observations of bringing the consumptions of gas and sperm to the agreed standard.

To the consumer of gas the evident inference from the data here presented is that where it is important to obtain a maximum of economical effect from the consumption of a given volume of illuminating gas, this result is best obtained by the use of burners of ample flow.

Where a moderate light of equal diffusion is required over a large space, as in public rooms, it may be expedient to use numerous small jets; but when the maximum intensity obtainable from a given volume of illuminating gas is desired, intensity burners of large consumption are plainly indicated.

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8. On the Composition of the Acid Oxalates of Potassium, Ammonium and Sodium. By William Ripley Nichols, of Boston, Mass.

Binoxalate of Potassium.

The composition of this salt was formerly held to be expressed by the formula* $KO, C_4 O_6 + 3 HO$ ($Є_2 KH Θ_4 + aq.$)

* On the authority of Graham, Phil. Tr. 1837, 50.

and this formula is still given by Gmelin, Watts, and others, as that of the commonly occurring salt.

Rammelsberg,* on the other hand, describes the salt obtained by neutralizing a certain quantity of oxalic acid with carbonate of potassium, and adding an equal amount of oxalic acid, as corresponding to the formula $2\,(KO,\ C_4\,O_6) + 3\,aq.$ $[4\,(\bar{C}_2\,KH\Theta_4) + aq.]$

Marignac,† having afterwards partially analyzed this salt, concluded that the correct formula was $C_4\,KH\,O_8$ $(\bar{C}_2\,KH\Theta_4)$ and that the crystals contained no water of crystallization. He differed from Rammelsberg as to the system to which the crystals should be referred, and the latter afterwards ‡ acknowledges the correctness of Marignac's views as to the crystalline form and, without repeating the analysis of the salt, seems satisfied to accept the formula assigned by Marignac.

I have prepared this salt in the manner indicated by Rammelsberg, and find that its composition agrees with the formula originally given by him.

	Calculated.		Found. I.	II.	III.	IV.	V.	VI.	VII.	VIII.	Mean.
$2\,K_2\Theta$	188.44	35.53	—	—	—	35.55	35.05	—	—	—	35.30
$4\,\bar{C}_2\Theta_3$	288.00	54.29	55.35	55.67	55.76	—	—	—	—	—	55.76
$3\,aq.$	54.00	10.18	—	—	—	—	—	10.16	10.39	10.58	10.34
$4\,(\bar{C}_2\,KH\Theta_4) + aq.$	530.44	100.00									

In these analyses I determined the potassium as carbonate by igniting a portion of the finely powdered crystals in a covered platinum crucible, raising the heat very gradually in order to avoid loss by projection, to which, as Marignac hints, this salt is particularly liable. The oxalic acid was determined by titration with a solution of permanganate of potassium standardized against pure oxalic acid. The hydrogen was determined by igniting the salt in a combustion tube, in a stream of dry air, and collecting the water in a weighed chloride of calcium tube.

Rammelsberg's formula demands.		Rammelsberg's own figures were.		
$2\,KO$	35.53	36.41	35.22	35.36
$2\,C_4\,O_6$	54.29	55.31	54.32	54.00
$3\,aq.$	10.18	—	—	—
	100.00			

Marignac's formula demands.		Marignac's own figures were.
KO	36.78	36.35 (Mean of four.)
$C_4\,O_6$	56.19	55.86
HO	7.03	—
	100.00	

* Pogg. Ann. XCIII, 24 (1854).

† Mém. de la Soc. d. Phys. et d'Hist. Nat. de Genève, T. XIV, part I. (1855).

‡ Supplement zu dem Handbuch der krystallographischen Chemie. Leipzic, 1857, s. 81.

Had Marignac determined the hydrogen in his salt, he would have found his formula to be inadmissible.

In regard to the acid oxalate described by Graham (loc. cit.),* it is extremely doubtful whether there be such a salt. Rammelsberg† doubts its existence, and I have myself been unable to procure it. I added to a hot solution of a known quantity of oxalic acid, half the carbonate of potassium necessary to neutralize it. The crystals which formed in the hot solution (A), those deposited from the solution at the ordinary temperature (B), as well as those deposited when the solution was artificially cooled to a considerably lower temperature (C), proved to be the quadroxalate $Ȼ_2 KHϴ_4, Ȼ_2 H_2 ϴ_4, + 2\,aq.$

	Calculated.		Found. A. I.	A. II.	A. III.	B. I.	B. II.	C. I.
$K_2ϴ$	94.22	18.54	—	—	18.41	18.53	—	—
$4\,Ȼ_2ϴ_3$	288.00	56.67	56.23	56.52	—	—	56.86	56.33
$3\,H_2ϴ$	54.00	24.79	—	—	—	—	—	—
$4\,aq.$	72.00							
$2\,(Ȼ_2 KHϴ_4, Ȼ_2 H_2 ϴ_4, + 2\,aq.)$	508.22	100.00						

In these estimations the potassium was determined as carbonate, by ignition, and the oxalic acid by titration, as in the preceding case.

I analyzed several samples of commercial "binoxalate of potash," but each sample proved to be quadroxalate.

BINOXALATE OF AMMONIUM.

This salt was prepared by neutralizing a certain quantity of oxalic acid with ammonia-water, and then adding an equal quantity of oxalic acid. Analysis showed the composition of the salt to be $2\,(Ȼ_2\,(NH_4)\,Hϴ_4) + aq.$

	Calculated.		Found. I.	II.	III.	IV.	Mean.
$(NH_4)_2ϴ$	52	22.41	—	—	21.54	—	21.54
$2\,Ȼ_2ϴ_3$	144	62.08	61.35	61.57	—	—	61.46
$H_2ϴ$	18	7.75	—	—	—	—	—
$aq.$	18	7.76	—	—	—	7.50	7.50
$2\,(Ȼ_2\,(NH_4)\,Hϴ_4) + aq.$	232	100.00					

	Calc.	Found.
* KO	32.24	32.34
C_4O_6	49.28	—
$3\,HO$	18.48	—
	100.00	

Graham deduced this formula from the amount of carbonate of potassium left on ignition (47.45 per cent.).

† Pogg. Ann. XCIII, 24 (supra cit.).

The formula usually given in text-books on chemistry (Gmelin, Watts, etc.), is $C_4\,(NH_4)\,H\,O_8 + 2\ aq.$ $\text{€}_2\,(NH_4)\,H\,\Theta_4 + aq.$, that is, with one more molecule of water than I find to be the case. For this formula the calculated percentages would be

$(NH_4)_2\,\Theta$	52	20.80
$2\,\text{€}_2\,\Theta_3$	144	57.60
$H_2\,\Theta$	18	7.20
$2\ aq.$	36	14.40
100.00	250	

Anderson* says that the binoxalate of ammonium may be obtained by mixing equivalent quantities of chloride of ammonium and oxalic acid (regarded as monobasic; $C_2\,O_3,\ H\,O + 2\ aq. = 63$), and gives as the formula $2\,C_2\,O_3,\ N\,H_4\,O,\ 2\,H\,O$ $[2\,(\text{€}_2\,(NH_4,\ H\,\Theta_4) + aq.]$.

He determined the oxalic acid alone, and, from the data that he gives, it would appear that, instead of the binoxalate, he really obtained the quadroxalate mentioned below.

Percentage of $\text{€}_2\,\Theta_3$ in his salt.	in the acid salt above.	in the hyperacid salt below.
61.92	62.08	61.80.

I found that by adding a hot solution of 53.5 grm. (1 eq.) chloride of ammonium ($NH_4\,Cl$), to a hot solution of 63 grm. ($\frac{1}{2}$ eq.) of crystallized oxalic acid ($\text{€}_2\,H_2\,\Theta_4 + 2\,aq.$), there were deposited on cooling crystals of the hyperacid salt of the formula $\text{€}_2\,(NH_4)\,H\,\Theta_4,\ \text{€}_2\,H_2\,\Theta_4, + 2\,aq.$

	Calculated.		Found. I.	II.	III.	IV.	V.	VI.	Mean.
$(NH_4)_2\,\Theta$	52	11.16	11.12	11.29	—	—	—	—	11.20
$4\,\text{€}_2\,\Theta_3$	288	61.80	—	—	61.14	61.14	61.09	—	61.12
$3\,H_2\,\Theta$	54	11.59	—	—	—	—	—	—	—
$4\,aq.$	72	15.45	—	—	—	—	—	15.81	15.81
$2\,(\text{€}_2\,(NH_4)\,H\,\Theta_4,\ \text{€}_2\,H_2\,\Theta_4, + 2\,aq.)$	466	100.00							

In these analyses the ammonium was determined as chloroplatinate of ammonium, the oxalic acid by titration, and the water of crystallization by drying at 100° $C.$, until the weight remained constant.

BINOXALATE OF SODIUM.

Anderson (loc. cit.) says that, by dissolving equivalent proportions of oxalic acid (equiv. = 63) and chloride of sodium (equiv. = 58.5), in hot water, crystals of this salt are obtained

* Qu. Jour. Ch. Soc. I. 231 (1849).

on cooling the solution. He gives the formula for the same $Na\,O, 2\,C_2\,O_3 + 4\,H\,O\,[2\,(€_2\,Na\,H\,Θ_4) + 3\,aq.]$

I found that crystals of the binoxalate were deposited from such a mixture, but that they answered to the commonly received formula $C_4\,Na\,H\,O_8 + 2\,aq.\ [€_2\,Na\,H\,Θ_4 + aq.]$

	Calculated.		Found. I.	II.	Mean.
$Na_2\,Θ$	62	23.85	—	—	—
$2\,€_2\,Θ_3$	144	55.38	55.19	55.24	55.22
$H_2\,Θ$ } $2\,aq.$ }	54	20.77	—	—	—
$2\,(€_2\,Na\,H\,Θ_4 + aq.)$	260	100.00			

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9. On the Solubility in Water of the Oxalates of Sodium, Potassium and Ammonium, at the ordinary temperature of the Air. By William Ripley Nichols.

In determining the solubilities of the salts experimented upon, the method employed to obtain solutions, saturated at the observed temperatures, was as follows:—Considerable quantities of the salts operated upon, several times as much as would be likely to dissolve in the amount of water used, were put into glass-stoppered bottles, which were then half filled with distilled water, and placed in a pan of water so as to be immersed up to the necks. The operation was carried on in a room where the variation of temperature was slight, such variation being noted by means of a thermometer suspended in the pan of water.

The bottles were shaken conscientiously at frequent intervals for two or three days, and, finally, portions of the solutions were filtered through dry filters, into tared flasks, and weighed. As a rule the thermometer had indicated a constant temperature for several hours previous to the filtration.

The amount of oxalic acid in the weighed solution, was determined by titration with permanganate of potassium, standardized against pure oxalic acid and from this result the amount of salt dissolved was calculated.

In every case but one, the salts were prepared by myself, and in every case the character and purity of the salt in question was ascertained by titrating a weighed portion of the dry salt with the standard solution of permanganate of potassium.

Oxalate of Sodium.

$$C_2 Na_2 O_4.$$

This salt was prepared by neutralizing a hot solution of oxalic acid with pure carbonate of sodium. The oxalic acid used in preparing this, as well as the other salts, left upon ignition 0.03 per cent. ash.

	Calculated.		Found. I.	II.	Mean.
$Na_2 O$	62	46.27	—	—	—
$C_2 O_3$	72	53.73	54.06	53.62	53.84
	134	100.00			

Solubility.—Temperature at time of filtration, . . 13° C.
Temperature had varied during solution from 11° to 13.5°.

100 parts of the solution saturated at 13° contain:

I.	II.	III.	Mean.	
3.063	3.066	3.047	3.059	parts of the crystallized salt.

Or, 100 parts of water at 13° dissolve 3.156 parts of the crystallized salt.

Or, 1 part of the crystallized salt is soluble in 31.6 parts of water at 13°.

This agrees with the determination of Souchay and Lensen* who say that one part of the salt dissolves in 31.1 parts of water at 15.5°.

Pohl† says that one part of salt dissolves in 26.7 parts of water at 21.8°.

Binoxalate of Sodium.

$$C_2 Na H O_4 + aq.$$

This salt was prepared by adding 58.5 grm. (1 eq.) of chloride of sodium in solution, to a hot solution of 63 grm. ($\frac{1}{2}$ eq.) of crystallized oxalic acid, and recrystallizing the product deposited from the solution when cold.

* Ann. Ch. u. Ph. XCIX. 33 (1856).
† Wien. Acad. Ber. VI. 596 (1851).

	Calculated.		Found. I.	II.	Mean.
$Na_2Θ$	62	23.85	—	—	—
$2\,Є_2Θ_3$	144	55.38	55.08	55.35	55.22
$3\,H_2Θ$	54	20.77	—	—	—
$2(Є_2NaHΘ + aq.)$	260	100.00			

Solubility.—Temperature at time of filtration, . . 10°.
Temperature had varied between 5° and 10°.

100 parts of the solution saturated at 10° contain:

I.	II.	III.	Mean.	
1.40	1.39	1.55	1.45	parts of the crystallized salt.

Or, 100 parts of water at 10° dissolve 1.48 parts of the crystallized salt.

Or, one part of the crystallized salt dissolves in 67.57 parts of water at 10°.

Souchay and Lensen (loc. cit.) say that one part of the salt dissolves in 60.3 parts of water at 15.5°.

Oxalate of Potassium.

$$Є_2K_2Θ_4 + aq.$$

This salt was prepared by neutralizing a commercial sample of quadroxalate of potassium with carbonate of potassium and recrystallizing twice.

	Calculated.		Found. I.	II.	III.	Mean.
$K_2Θ$	94.22	51.14	—	—	—	—
$Є_2Θ_3$	72.00	39.08	38.99	39.63	38.53	39.05
$H_2Θ$	18.00	9.78	—	—	—	—
$Є_2K_2Θ_4 + aq.$	184.22	100.00				

Solubility.—Temperature at time of filtration, . . 16°.
Temperature had varied between 12° and 16°.

(The temperature had remained at 16° for several hours.)

100 parts of the solution saturated at 16° contain:

I.	II.	Mean.	
24.73	24.89	24.81	parts of the crystallized salt.

Or, 100 parts of water at 16° dissolve 32.99 parts of the crystallized salt.

Or, one part of the crystallized salt is soluble in 3.03 parts of water at 16°.

Binoxalate of Potassium.

$$4\,(C_2KHO_4) + aq.$$

This salt was prepared as stated in the preceding paper.

Solubility.—Temperature for three hours preceding filtration, 8°
Temperature had varied between 8° and 10.5°.

100 parts of the solution saturated at 8° contain:

I.	II.	III.	Mean.	
3.680	3.681	3.668	3.676	parts of the crystallized salt.

Or, 100 parts of water at 8° dissolve 3.816 parts of the crystallized salt.

Or, one part of the crystallized salt is soluble in 26.21 parts of water at 8°.

Quadroxalate of Potassium.

$$C_2KHO_4,\ C_2H_2O_4 + 2\,aq.$$

This salt was prepared by recrystallizing a sample of commercial "binoxalate of potash".

	Calculated.		Found. I.	II.	III.	IV.	Mean.
K_2O	94.22	18.54	—	—	—	18.45	18.45
$4C_2O_3$	288.00	56.67	55.80	55.63	55.77	—	55.73
$3H_2O$	54.00	24.79	—	—	—	—	—
4 aq.	72.00						
$2\,(C_2KHO_4,\ C_2H_2O_4,\ +2\,aq.)$	508.22	100.00					

Solubility.—Temperature at time of filtration, . . 13°.
Temperature had varied between 12° and 14.5°.

100 parts of the solution saturated at 13° contain:

I.	II.	Mean.	
1.774	1.784	1.779	parts of the crystallized salt.

Or, one part of the salt is soluble in 55.25 parts of water at 13°.

Or, 100 parts of water at 13° dissolve 1.81 parts of the crystallized salt.

Pohl (loc. cit.) says that 100 parts of water at 20.6° dissolve 4.957 parts of the salt dried at 100° (5.775 parts of the crystallized salt), which would go to show that the solubility must increase rapidly with the temperature.

OXALATE OF AMMONIUM.

$$C_2(NH_4)_2\Theta_4 + aq.$$

This salt was prepared by neutralizing a hot solution of oxalic acid with ammonia-water.

	Calculated.		Found. I.	II.	III.	Mean.
$(NH_4)_2\Theta$	52	36.62	—	—	—	—
$C_2\Theta_3$	72	50.70	50.72	50.92	51.48	51.04
$aq.$	18	12.68	—	—	—	—
$C_2(NH_4)_2\Theta_4+aq.$	142	100.00				

Solubility.—Temperature at time of filtration, . . 15°.
Temperature had varied between 13.5° and 15°.

100 parts of the solution saturated at 15° contain:

I.	II.	Mean.	
4.028	4.076	4.052	parts of the crystallized salt.

Or, 100 parts of water at 15° dissolve 4.22 parts of the crystallized salt.

Or, 1 part of the crystallized salt dissolves in 23.69 parts of water at 15°.

I verified the statement of Heintz* that this salt is less soluble in a solution of chloride of ammonium than in pure water. I added chloride of ammonium to a concentrated solution of the salt, and there were deposited small crystals which gave by titration 50.93 per cent. $C_2\Theta_3$, showing that they were actually the normal oxalate.

BINOXALATE OF AMMONIUM.

$$2[C_2(NH_4)H\Theta_4] + aq.$$

This salt was prepared as stated in the previous paper.

Solubility.—Temperature at time of filtration, . . 11.5°.
Variation of temperature—slight.

100 parts of the solution saturated at 11.5° contain:

I.	II.	III.	Mean.	
5.89	5.91	5.88	5.896	parts of the crystallized salt.

Or, 100 parts of water at 11.5° dissolve 6.26 parts of the crystallized salt.

Or, 1 part of the crystallized salt is soluble in 15.97 parts of water at 11.5°.

* Zeitsch. f. d. ges. Naturw. XX. 29.

In order to ascertain whether this salt dissolved unchanged, a portion of that remaining undissolved, was titrated with the standard permanganate, and the percentage of oxalic acid found agreed with that of the original salt.

Quadroxalate of Ammonium.

$$C_2(NH_4)HO_4,\ C_2H_2O_4 + 2\,aq.$$

This salt was prepared by adding to half an equivalent of oxalic acid ($C_2H_2O_4 + 2\,aq.$) an equivalent of chloride of ammonium (NH_4Cl), as described in the preceding paper.

Solubility.—Temperature at time of filtration, 7.75°.
Temperature had varied but slightly.

100 parts of the solution saturated at 7.75° contain:

I.	II.	III.	Mean.
2.45	2.46	2.46	2.46 parts of the crystallized salt.

Or, 100 parts of water at 7.75° dissolve 2.52 parts of the crystallized salt.

Or, one part of the crystallized salt is soluble in 39.68 parts of water at 7.75°.

Oxalic Acid.

$$C_2H_2O_4 + 2\,aq.$$

A portion of pure crystallized oxalic acid was taken, and its solubility determined to be as follows:

Solubility.—Temperature at time of filtration, 14.5°.
Variation of temperature—slight.

100 parts of the solution saturated at 14.5° contain:

I.	II.	III.	Mean.
8.668	8.777	8.754	8.733 parts of the crystallized salt.

Or, 100 parts of water at 14.5° dissolve 9.56 parts of the crystallized salt.

Or, one part of the crystallized salt is soluble in 10.46 parts of water at 14.5°.

THE FOREGOING RESULTS MAY BE TABULATED AS FOLLOWS:—

Name of Salt.		Temperature.	100 parts of the saturated solution contain parts salt.	100 parts water dissolve parts salt.	1 part salt soluble in parts water.
Oxalic Acid, ...	$Є_2 H_2 Θ_4 + 2$ aq.	14.5°	8.73	9.56	10.46
Oxalate of Sodium, ...	$Є_2 Na_2 Θ_4$	13°	3.06	3.16	31.60
Binoxalate of Sodium, . .	$Є_2 Na H Θ_4 +$ aq.	10°	1.45	1.48	67.57
Oxalate of Potassium, . .	$Є_2 K_2 Θ_4 +$ aq.	16°	24.81	32.99	3.03
Binoxalate of Potassium, .	$4 (Є_2 K H Θ_4) +$ aq.	8°	3.68	3.82	26.21
Quadroxalate of Potassium,	$Є_2 K H Θ_4, Є_2 H_2 Θ_4 + 2$ aq.	13°	1.78	1.81	55.25
Oxalate of Ammonium, . .	$Є_2 (NH_4)_2 Θ_4 +$ aq.	15°	4.05	4.22	23.69
Binoxalate of Ammonium, .	$2 (Є_2 (NH_4) H Θ_4) +$ aq.	11.5°	5.89	6.26	15.97
Quadroxalate of Ammonium,	$Є_2 (NH_4) H Θ_4, Є_2 H_2 Θ_4 + 2$ aq.	7.75°	2.46	2.52	39.68

B. NATURAL HISTORY.

GEOLOGY AND PALÆONTOLOGY.

1. On the Cretaceous Age of Silver-deposits in Chihuahua, Mexico, by James P. Kimball, of New York.

A tour made last autumn and winter, from the Texas coast to the Grand Sierras, or Cordilleras of Mexico, near the thirtieth parallel, afforded a few geological observations of such importance that I venture to lay them before the Association. It will not be necessary on this occasion to enter into minute description. But the few points I have now to offer will, I trust, serve partly to bridge the gap in the knowledge of the country between the Rio Grande on the east, and the Cordilleras; and to rectify a number of errors which mainly make up the meagre geological part of what otherwise is an interesting brief account of Chihuahua—that of Dr. Wislezenus of Col. Doniphan's Expedition in 1846–7, whose report, together with that of the Mexican Boundary Survey, contain the only references of any consequence to the geology of Chihuahua, to be found in any language—so seldom has any other part of the state than the High Sierras been visited by scientific travellers, while even that part, comparatively accessible from the Pacific Coast, is, along with the rest of the state, less known than any other portion of Mexico.

The Cretaceous formation which overspreads Central Texas, where it has been carefully studied by Professor Roemer and Dr. B. F. Shumard, is traced westward under pretty uniform conditions, beyond the Pecos, forming the table-land of the intervening belt, until, in the Limpia Mountains, its familiar features disappear and a new order of things supervenes. Up to this line the geology of Texas has been fixed. Within the great bend of the Rio Grande, the water-shed between the

Pecos and that river, presents geological features which have been described by travellers whose studies have not extended far into Chihuahua. While these features are anomalous in Texas, they prevail in Chihuahua, and are to be regarded, as I propose to show, as an extension of the same conditions which characterize the whole of the eastern slope of the Cordilleras in the north-eastern part of Mexico.

Topographically considered, the whole surface of Texas is a part of the slope of the Mexican Cordilleras. From the Gulf of Mexico to the Limpia Mountains, which are only a part of a long dividing range between the Pecos and the Rio Grande, extending from New Mexico, there is a constant acclivity toward the west, which, gradual, until the Alluvial, Diluvial, and Tertiary coast margin is passed, becomes very marked as soon as the Cretaceous rocky strata set in. From San Antonio, five hundred and seventy-nine feet above the sea, which is near the line between the Tertiary and Cretaceous formations, the Cretaceous surface rises to three thousand and ninety-eight feet, at Leon Springs (some two hundred and eighty miles farther west), and to near four thousand feet before the fossiliferous Cretaceous strata are covered by the rocks which form the Limpia Mountains. These mountains present a thickness from eight hundred to one thousand feet of a rock of peculiar character, which has been described in the Report of the Mexican Boundary Survey, as a porphyry, and as furnishing a "coarse granitic aggregate of which adularia forms a large part," both of which varieties, together with compact quartz, are referred to an *igneous* source: whence the inference is drawn that the Limpia Mountains, as well as what is mentioned as a continuation of the same range—the Sierra Rica, in Mexico—are an axis of elevation. From all these points I have to dissent.

First, as to the lithological character of the Limpia Mountains. Although clearly porphyritic in its structure, and containing (yet rarely) small segregations rather than crystals of orthoclase, it presents, to the eye at least, the novel condition of a non-feldspathic matrix, and therefore, in the restricted sense of the term, can hardly be called a porphyry. The matrix is a ferruginous compact quartz, through which are diffused, besides orthoclasic, minute quartz, crystallizations. Hence this rock is rather to be characterized as a porphyritic quartzite. These characters are uncommonly pronounced where the rock first sets in, namely, at Hackberry Ponds.

This is an extension of the same formation which overspreads a large part of Chihuahua—a fact ascertained by continuity and relative stratigraphical position, rather than by lithological uniformity. The rock is disposed in strata which are gently undulating. Its ferruginous character causes it to weather excessively—especially where its consistency is not very dense. In the eroded valley of the Limpia, bold cliffs of it are seen weathered into massive picturesque columns of a dark ochreous color. The ferruginous coloring matter is present in the interior as red oxyd. At Fort Davis a quarry in this rock furnishes a building-stone, called by the Mexicans *cantera*. Here its porphyritic characters have entirely disappeared, its quality being a loose alumino-silicious aggregate. It may be cut with a saw, and much resembles the building-stone or *cantera* in use in the city of Chihuahua, and quarried in metamorphic strata at the same horizon as this. The same rapid transitions in consistency, yet a constant uniformity of its peculiar type, are witnessed throughout the wide development of this rock which I will afterwards designate as *cantera* (a Spanish word, simply meaning, however, building-stone). Its metamorphic character is indicated by phenomena already mentioned; while the formation of the Limpia Mountains, by the mere accumulation of nearly level strata, is so distinct as to give no appearance of an axis of elevation. The relation of the metamorphic *cantera* to the underlying Cretaceous limestone, is revealed by a quarry below the level of the plain, which supplies the lime-kiln of the post.

From the head of the Limpia, near Fort Davis, to Presidio del Norte, on the Rio Grande, there is a gradual descent of over twelve hundred feet. The *cantera*, which in high bluffs skirts the valley-plains for some distance, comes down to the level of the road some sixty miles this side of the Rio Grande, and is succeeded by fossiliferous limestone, some thirty miles farther. The river terraces, five or six in number, begin ten to twelve miles from the river. Passing a corresponding set of terraces on the west side, the surface begins immediately to ascend in impure limestones and calcareous shales, dipping 15° to the north-west. The limestone, of a sandy character, contains numerous fossils, some in great abundance, among which are forms of Cretaceous fauna known in Texas, viz.: *Exogyra plicata*, *Scalaria Texana*, *Natica Texana*, *Lima Wacoensis*, together with species of *Ammonites Ostrea*, and *Inoceramus*, and fish teeth. The Sierra de las Cuestas, which form the bluffs of the Rio Grande valley proper, and the continuation of these mountains to the south—the Sierra Rica—are made up of this Cretaceous series. The latter mountains are the centre of a slight dislocation which imparts to the series north-west dips from 15° to 20°, as seen in the mountains themselves, where the succession is, (1) lower shales and fissile slates; (2) blue limestone with *inoceramus;* (3) upper fissile calcareous shales; (4) arenaceous cavernous limestone with *inoceramus;* (5) a coarse metamorphic rock made up of crystalline quartz and orthoclase. The Sierra Rica lode, bearing sulphurets of silver and of the base metals is encased in this fossiliferous series, its highest outcrop being on the crest of a prong from which the quartzose rock of the neighboring summits has been stripped by denudation.

The Conchos River valley seems to occupy an anticlinal axis of an elevation of the Cretaceous limestone series, the western part of which has been obliterated by river erosion, and only its eastern part preserved in the form of a gentle monoclinal, with a dip to the south-east. Such, at least, appears to be the structure of the Cuchillo Parado, the Chupaderos and Chorreras Mountains, as seen from the Chihuahua road, which keeps along their base, across the Jornada or great barren plain of Rosales. At its western end, where the bordering mountains draw together at the gap of the Gallina, between the mountains of that name on the east, and the Chupaderos on the west, a sudden increase of the south-east dip of the limestone, lets in the summit *cantera* with a sufficient thickness to form the mountains to the south and east of the Jornada; while westward the Chorreras range continues to raise the limestone in heights, from three hundred to five hundred feet, until obliterated at the bend of the Conchos, which cuts across it, and whose valley here spreads out some twenty-five miles, under the erosive influence of the close confluence with it of the Sacramento and Rosales branches. In this interval the limestone passes below the surface, and the metamorphic *cantera* sets in to form the elevations which, in north and south ranges from five to ten miles broad, separated by wide and longitudinally remarkably continuous valleys or valley plains, succeed each other all the way to the culminating line of the Cordilleras, and thus characterize their eastern flank in this latitude. An acclivity of the whole surface, toward the west, is very marked —all the way indeed from the Rio Grande, the road rising from two thousand seven hundred and seventy-nine feet above the sea at Presidio del Norte, to four thousand six hundred and forty at the city of Chihuahua, and six thousand two hundred and seventy-five feet at Cusihuiriachic, the culminating point (Cumbres de Jesus Maria) being only four hundred and fifty-seven feet higher than the Peak of Cusihuiriachic, or eight thousand three hundred and seventy-five feet above the sea.

This longitudinal succession of narrow ranges and wide champaign valleys, gives rise to a peculiar topography whose phenomena are mainly those of diluvial and alluvial erosion. The north-east dip of the *cantera*, though liable to local variations, is, on the whole, scarcely greater than the incline of the general surface. It is always seen under rapid disintegration, especially its more ferruginous portions. Its fine detritus overspreads all the valley-plains in such thick and ever growing accumulations, as often to fill up former water-courses. Those which do not contain running streams, all have a basin configuration: that is, a depressed portion toward the middle; while toward their borders the foot hills of the intervening mountains are seen in all stages of degradation. Thus the valleys are widening at the expense of the mountains; and their plain-like character is increasing under the determination of the finer detritus toward their former channels, now often dry except during the brief rainy season. These observations are true equally throughout the development of the metamorphic *cantera* in western Texas as throughout Chihuahua, north of the twenty-eighth parallel which I did not pass to the south. These phenomena, replete with interest, I can take occasion now only thus briefly to note. One other observation in a general way, however, I may add. It is that these and closely related phenomena, point to the elevation of the whole eastern Cretaceous slope of the Cordilleras, comprising Chihuahua and the greater part of Texas, since the outlines of the present topography were determined by water-courses, now either entirely obliterated, or else taking an inconsiderable part in the river system of this extensive area.

It will be understood that what I have distinguished as *cantera* forms, with the exception of what I have called the limestone range, along the western base of which flows the Conchos, as much of the surface of Chihuahua as comes under observation in traversing the state by way of Presidio del Norte and the city of Chihuahua, as far west as Cusihuiriachic — a point pretty well up toward the summit of the Cordilleras, — and as far north as El Paso by the road from the city of Chihuahua. This *cantera* overlies the Cretaceous fossiliferous limestone, and is, I regard, a metamorphic upper member of that formation.

I now come to the point the importance of which alone could warrant me in troubling you with local descriptions, namely: (1) that the seat of the principal known silver deposits of eastern and middle Chihuahua, is the Cretaceous fossiliferous limestone; and (2) that the seat of other great deposits is the overlying *cantera;* also, I am disposed to think, of Cretaceous age, a metamorphosed superior zone of that formation.

The Santa Eulalia range of mountains, one of the characteristic ranges already described, lies about half way between the Conchos and the city of Chihuahua. These mountains are mainly composed of quartziferous cantera, somewhat porphyritic; but in the heart of them rises a boss of the underlying limestone in which are found the old Spanish silver mines of Santa Eulalia. Coextensive with this elevation of the limestone, and confined to it, is the mining ground comprising an area of not more than five square miles. From 1705 to 1791, during a period of 86 years these mines, according to official records, yielded $100,000,000 on which the *quinto* or King's fifth was paid to the royal exchequer. To this product from a fifth to a third more is to be added for yield clandestinely obtained. The boss rises some eight hundred feet above the bordering plains and some five hundred feet above its immediate base. It is scored by water-courses, giving rise to deep cañons, in the bottoms of which and along whose bluffs the mines have been opened wherever access by bridle paths could be got, while the tops of the limestone hills have likewise afforded entrance. The silver deposits are of a very irregular description. The bulk of them is an invisible chlorid of silver minutely diffused through a decomposed and ferruginous matrix, sometimes associated with a low grade of argentiferous galena, and sometimes with salts of lead, and varying in richness from twenty-five dollars to one hundred and twenty-five dollars per ton. Occasionally rich pockets are found in which occur visible sulphurets of silver, and crystals of horn-silver. This argentiferous material is found with quartz, gypsum and calcspar, disposed between planes of stratification, or oblique joints in the beds; or in irregular decomposed courses in massive beds: sometimes thoroughly disseminated through a portion of a bed or a number of beds, and occasionally in drusy cavites. No particular choice seems to be given to any bed or any set of beds, but all alike have been, and are still, worked with much the same results. The whole series of beds is exceedingly cavernous. Natural caves, into which workings lead, were shown me capable of holding, it is said, the great cathedral of Chihuahua. This remarkable and unique character of the deposits has led both to vertical and horizontal workings. The same beds which are entered by caverns in the elevated portion, are reached by shafts in the bordering cañons, as the dips bear them from above water level. All the beds are more or less fossiliferous. *Inoceramus*, *Radiolites*, *Pecten*, and a well preserved coral, together with other Cretaceous forms, being found outside any of the mines, but generally on weathered surfaces from which it is difficult to remove them.

Besides the silver deposits in the Cretaceous limestone of Sierra Rica and Santa Eulalia, the intermediate limestone range at many points sustains a similar metalliferous character—argentiferous galena being well known in Mexico to occur in the Cuchillo Parado, in the Chupaderos and the Chorreras; while, in Texas, I saw in the possession of Major

General Merritt, stibnite and galena from the Chanate Mountains—probably from the same fossiliferous Cretaceous horizon—inasmuch as I have a *Pecten* of that period from the same locality.

Another slight isolated elevation of the limestone, on a prolongation of the axis of the forementioned range, occurs low toward the base of the Sierra de Magistral between the city of Chihuahua and Cusihuiriachic. This locality is also well known as an old mining ground. The excellent silver mines of Corralitas, according to the Mexican Boundary Survey, are likewise in blue limestone, and though its age has not been determined, I may venture to predict it will likewise prove to be Cretaceous. (Rep. Mex. Bound. Surv. II, p. 1, Vol. I, 12.)

Throughout its development, the *cantera* of the mountains, or the metamorphic Cretaceous, as I believe it to be, also sustains a highly metalliferous character. With the exception of the limestone range (including the localities already mentioned) it is the seat of all the silver deposits within the section of country visited by me as far west as Cusihuiriachic, and undoubtedly considerably farther, as evidences taken at second hand tend to show. Gold as well as silver occurs in this formation.

The extensive old Spanish silver mines of Cieneguilla richly affording horn-silver in brecciated fissures traversing the *cantera* are located some sixty-five miles south-west of the city of Chihuahua. Some fifteen miles still farther south-west are the remarkable mines of Cusihuiriachic. The mining village of that name has an altitude of six thousand two hundred and seventy-five feet. Above it rises the *bufa* of the same name one thousand six hundred and forty-three feet. The flanks of this mountain, whose outlines have been determined by erosion, are traversed by noble fissures filled out with a complex vein-stuff of great richness in silver. A number of specimens, selected, but averaged according to grade, collected and assayed by myself, yielded from one hundred and forty dollars to thirteen hundred and fifty dollars per ton. These mines, under a practice both rude and desultory, have produced, in the course of two centuries, some $100,000,000. The quartziferous character of the *cantera* here predominates.

Here, as elsewhere, the more exposed portions exhibit the most strongly marked metamorphic aspect, as if its metamorphism had been effected by molecular and chemical changes of an alumino-silicious or felspathic, and perhaps somewhat calcareous and magnesian, aggregate, under the influence of the atmosphere and percolating waters. As tending to such a view, it will be necessary only to instance, besides its stratigraphical conditions and attitude, the apparent elimination of its more soluble ingredients as indicated both by the vesicular character of weathered surfaces, and by the remarkable fact that in all low places wherever the *cantera* is extensively developed, a rubble is found cemented by a carbonated base. This cementing material seems to have been derived from mineral percolations springing from the surperincumbent rocks. Such a concrete is found also under the detritus which, supplied by the *cantera* of the mountains, covers the valley-plains. Another observation bearing on this point, is the frequent occurrence of the soft earthy aggregate, or the typical *cantera* (building-stone) as a *portion* of the formation, particularly at the base of mountains, below or near natural drainage. Moreover, the *cantera*, both hard and soft, indicates in part a detrital origin by its inclosure of breccia and pebbles, thus bearing out the general impression it gives of altered felspathic sediments.

Professor Gabb has recently called attention to the occurrence of Texan Cretaceous fauna in Sonora, where they were found by the late Mr. Rémond, as going to show a "water communication between the great Cretaceous sea that covered so much of what is now the central portions of our continent, on the one side, and the Pacific on the other. The recognition of the Cretaceous near Arivechi in Sonora, according to Mr. Gabb, was only the second locality reported of its occurrence in the whole area of Mexico, the other being in the State of Puebla.

Although the metamorphic Cretaceous is, through the labors of the California Survey, well known in numerous localities in the coast ranges, as a seat of gold deposits and the Jurassic, east of the Cordilleras within the territory of the United States, of silver deposits, the instances which I have just given, of the unequivocal fossiliferous Cretaceous, as well as a probable metamorphic and later zone of the same formation, as sources of silver ores, are the first, I believe, yet brought to light in North America. The only similar occurrence of which I am aware is in Chili, where, according to Mr. Rémond, calcareous and fossiliferous Cretaceous strata, the same as at Santa Eulalia, carry irregular silver deposits.*

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2. Remarks on the Age and Relations of the Metamorphic Rocks of New Brunswick and Maine. By George F. Matthew and L. W. Bailey, of the Geological Survey of Canada.

The sediments to which the following remarks chiefly relate, are embraced in the most southerly of three spurs projecting to the north-east, from that extensive tract of altered and often highly crystalline rocks which occupy the greater part of New England; and lie between the unaltered Silurian of New York on the one hand, and the New Brunswick coal field on the other.

Dr. Gesner, who made a geological survey of New Brunswick between the years 1838 and 1842, recognized the existence of granitic ridges in two of these spurs, and spoke of the slates in the central part of the Province as Cambrian, while those which lie near the Bay of Fundy were described as Silurian. Subsequently the late Dr. James Robb, of King's College, Fredericton, in a geological map published about 1850, and in an explanatory chapter in Johnston's "Report on the Agricultural Capabilities of New Brunswick," retained in the main this arrangement. In both cases it would appear that the classification was based upon the highly altered character of the rocks, and the paucity or entire absence of organic remains.

* See an article by the writer in the American Journal of Arts and Sciences, vol. xlviii, 1869, p. 378. Notes on the Geology of Western Texas and of Chihuahua Mexico.

Very little was added to our knowledge of these obscure and puzzling formations for a number of years. About the year 1858, one of the authors of this paper, Mr. George F. Matthew, began observations at the city of St. John, resulting in discoveries which enabled Dr. J. W. Dawson to pronounce some of the deposits in that vicinity to be of Upper Devonian age. At the beginning of the present decade, a Geological Survey of the State of Maine, under Prof. C. H. Hitchcock, was undertaken, through which a knowledge of the age of some of the metamorphic rocks adjoining New Brunswick was obtained.* In the following year the characters of the metamorphic belt eastward of St. John were observed during a reconnoissance made for the Government of New Brunswick by the authors of this paper, in connection with Prof. C. F. Hartt. We were enabled to show the existence of Laurentian, Huronian and Primordial strata in the tract examined, and of Upper Silurian in Charlotte County, as well as to indicate the wide distribution of Devonian sediments along the coast both east and west of the St. John River. A large area, however, of altered rocks between the latter and the border of Maine, still remained in great part unknown.

When the work of the Geological Survey of Canada was extended to this Province in 1868, the authors of this paper undertook the examination of the area to which reference has been made. The following remarks embrace the principal results obtained by us, together with a summary of the facts at present known with reference to the structure of this and the neighboring regions.

LAURENTIAN SERIES.

The assemblage of rocks referred to this system occupy an area surrounded by more recent formations, about forty miles long and eight broad, extending from Mace's Bay, an indentation of the Bay of Fundy, north-eastward into King's County. It is well exposed in the Narrows of the St. John River, a few miles northward of the city of the same name.

The succession in this series is much obscured by faults

* We shall have occasion to refer to the voluminous observations made in parts of the State farther west, by Prof. Hitchcock, in the sequel.

and overlaps, but exhibits a repetition of beds similar in mineralogical characters to that observable in the same series elsewhere. It presents a great body of gneiss, often granitoid in aspect, and includes one or more bands of crystalline limestone, constituting veritable limestone formations many hundred feet in thickness, interstratified, however, with beds of quartzite and diorite. The limestones are in some cases fine-grained, but in others very coarsely crystalline, and include workable beds of crystalline graphite. Some of the limestones are micaceous, and they are often mixed with a pale green translucent *serpentine*, closely resembling that found in the Laurentian limestones of other parts of the continent.

In the south-western part of Charlotte County there is another area and several small patches of granitic and syenitic gneiss,* with gneiss conglomerate, which, mainly on lithological grounds, we are disposed to refer to the same formation. Limestones, however, are much less abundant in these than in the Laurentian of St. John County.

Labrador Series.

Several hills of crystalline felspar rock, associated with hypersthene,† and in some cases with magnetic iron, are found lying between the Laurentian and the Cambrian in St. John County. Dr. Hunt, who has recently visited the locality, considers them to be identical with the anorthosite felspar rocks of the Labrador or Upper Laurentian series, and is disposed to refer them to that formation.

Cambrian or Huronian Series.

The rocks referred to this series are exposed in two bands extending north-eastward from near the city of St. John, each about thirty miles long and three or four wide. Their greatest observed thickness is about ten miles east of St. John, where

* The largest exposure of these syenites has hitherto been regarded as a part of the long granite ridge represented in maps heretofore made, as extending through the metamorphic area south of the coal-field. They are evidently much older than the slates around them, and are quite different from the Nerepis granites to which we shall have occasion to refer in the sequel. They are regarded by Dr. Hunt as true Laurentian rocks.

† The hypersthene of this rock is sometimes in cleavable crystals one or two inches across.

it is apparently not less than seven thousand feet. Westward this thickness becomes, within a short distance, very greatly reduced, and where the beds cross the St. John River does not exceed fifty feet. Beyond this point, in the same direction, they have not been recognized.

The beds of this series consist mainly of hard felspathic rocks, often approximating to a petrosilex. They are more or less quartzose and generally epidotic, and vary in color from pale greenish or reddish to gray and dark gray. With them are associated considerable masses of diorite. At the base are hard red quartzose conglomerates and red argillaceous sandstones, and at the summit softer red sediments of the same color. In the finer felspathic rocks the stratification is often obscure, but may generally be detected in the coarser beds, which, towards both the base and summit, become a conglomerate or breccia.

In many of its features this group of rocks recalls the Huronian series of Canada, with which its stratigraphical relations would seem to make it equivalent.

Lower Silurian.

This series includes about one hundred and fifty feet of slates (holding *Paradoxides*, *Conocephalites*, *Agnostus* and other *trilobites*, besides several genera of brachiopods), and an overlying mass, measuring not less than two thousand feet, of flags and slates containing *Lingulæ*, worm-burrows, &c. These rocks lie chiefly in a narrow valley, about thirty miles long and four miles wide, between the ridges of Cambrian rocks mentioned above, and to which they are conformable. They are well exposed about the city of St. John, beyond which they extend but a short distance to the westward. Strata of this age also cover portions of the Laurentian rocks north and west of St. John.

The basal portion of this formation, in which alone trilobites have been found, was pronounced by Professor Hartt (in 1865), in a preliminary notice embodied in our report of that year, to be truly primordial, and equivalent to the Etage *C* of Barrande in Bohemia, a conclusion confirmed by Mr. Billings, who pronounced these rocks to be the same with those of the

"Lower Lingula Flags" of Great Britain, and in all probability the same with the St. John Group of Newfoundland, and the *Paradoxides* beds of Braintree, Mass., while they represent an horizon lower than that of the Potsdam of New York. The latter may be in part represented by the upper members of the St. John Group.

Upper Silurian.

Rocks of Upper Silurian age have been recognized by their organic remains at Cobscook Bay in Washington County, Maine, and eastward of Passamaquoddy Bay in New Brunswick. The great metamorphism which these sediments have undergone, and the resemblance borne by many of them to those of the Devonian series, often render their recognition difficult, but about Passamaquoddy Bay, where they are best exposed, and where they mainly constitute the larger islands which here skirt the coast, they appear to consist of a thick series of gray and dark gray, sometimes sandy, shales, black fissile carbonaceous slates, felsites (frequently amygdaloidal), and heavy beds of diorite. They are almost everywhere inclined at high angles.

A mass of gray felspathic slates, diorites and light colored felsites, described in previous publications under the provisional name of the Kingston series, has been supposed to appertain to the group now under consideration. Our present knowledge is not sufficient to enable us, in the absence of fossils, to pronounce confidently as to the age of these rocks, but from the data possessed by us we are inclined to regard them as including both Upper Silurian and Lower Devonian beds, in this respect resembling a portion of the slates bordering the great coal-basin to the north, in which fossils, belonging near the base of the last named group, have been found, and which will be again referred to in the sequel.

From the Devonian series, to which the Kingston rocks bear some resemblance in lithological characters, they differ chiefly in their great uniformity over wide areas; for while the former (as will be presently shown) present very different aspects between the St. John River and the Passamaquoddy area, the latter, from the central portion of King's

County to where they disappear upon the coast, a distance of fifty miles, retain their peculiar features unaltered.

SILURO-DEVONIAN.*

That portion of the metamorphic area in southern New Brunswick not occupied by the rocks above described, consists of Siluro-Devonian strata and granite. Of these the latter forms a ridge of variable width, having Siluro-Devonian sediments on each side. The two together probably occupy three-quarters of the metamorphic country south of the coal-field.

Two principal divisions of these sediments may readily be distinguished on the south side of the granite ridge, viz.:—

		St. John County.	*Charlotte County.*
LOWER DIVISION.	*Lower.*	Limestones, felsites, &c., Conglomerates, local.	Felspathic slates.
	Upper.	Gray sandstones, black slates, "Dadoxylon sandstones."	Gray and black silicious slates.
UPPER DIVISION.	*Lower.*	Conglomerates and diorites, "Cordaite,"	Conglomerates, flags and diorites.
		Fine-grained slates and orthophyre, "do."	Gray and red orthophyre.
	Upper.	Conglomerate and slate. Granitoid grit.	Conglomerate and slate. Gritty felsites.
		Talcoid (?) slates and limestone (thin and perhaps local).	Talcose slates and limestone.

The basal part of the Lower division, which about St. John and to the eastward of it consists of hard conglomerate rocks, is represented in the western part of the county of the same name by felsites, limestones and conglomerate. A projecting ridge of Laurentian gneiss intervenes between the two deposits. Still farther west (as in Charlotte County) felspathic slates are found at this horizon.

Granites.—The mass of granite which extends from the Digdequash River in Charlotte County to the St. John River

*A more extended knowledge of the Devonian rocks than we possessed at the time that our Report on the Geology of southern New Brunswick was written, has led us to modify the classification of this series given there, and in an article on the Azoic and Palæozoic rocks of southern New Brunswick. The fossiliferous portion included in those publications under the name of "Little River Group," is now divided, the "Dadoxylon Sandstone," being associated with the Bloomsbury series under the new designation of the Lepreau division, and the "Cordaite shales" connected with the Mispeck or upper division. All the beds denominated "Upper Bloomsbury" in the older report are included under the head of "Lower Lepreau," and the "Mispeck" of the former publications in like manner is spoken of as "Upper Mispeck."

in Queen's County, is closely connected with the basal portion of this Siluro-Devonian series. These granites rise into hills of considerable altitude, and may be well seen on the Nerepis and Musquash Rivers which cross the middle of the granite range. At the former stream the central portion of the mass consists of fine-grained, tawny felspathic granite or eurite, porphyritic with numerous crystals of felspar and rounded grains of quartz. On either side of this lie coarse red granites, also porphyritic, and holding occasional rounded lumps of gray gneiss and granite, or rarely a boulder of quartz-rock. At this horizon in the granitic mass the rock is often loose in texture and distinctly laminated. This laminated structure results from the presence of numerous parallel planes in the rock, dipping at low angles. It is probable that these indicate half obliterated sedimentary layers, for, on the West Musquash River, the cliffs of this crumbling variety of granite present that irregularity of outline so often seen in sandstone deposits made up of beds of unequal hardness. The highest beds of granite contain but little quartz and mica and thus pass into crystalline felsites.

Age of the Granite.—The section of this mass of crystalline rocks exposed in the Nerepis valley exhibits their relations to the overlying slates very clearly. From the finer granites towards the centre of the mass we pass in descending this river to tawny syenite, with well defined hornblende crystals; this is followed by red syenite, which in its turn gives place to a syenitic rock in which the hornblende does not exhibit distinct crystals, but is in the form of dark earthy spots in the red rock. The deposit which overlies this is one of dark yellowish or reddish crystalline felsite; this differs from the syenite in holding a smaller proportion of quartz, but still contains much hornblende. In the upper one thousand (?) feet this rock is cryptocrystalline. Immediately upon this rests one hundred feet of somewhat slaty petrosilicious rock containing Siluro-Devonian fossil shells. These rocks are covered by one hundred and fifty feet of dark gray crystalline felsite and diorite, which in its turn is overlaid by eighty feet of dark gray silicious slates, holding shells of the same genera as those in the lower beds.

So far as can be judged from the exposures along this valley there appears to be a gradual passage from true granite through felsites to undoubted Siluro-Devonian rocks. Though the same transition has not been elsewhere so clearly marked, yet at several points along the southern border of the granitic area facts may be noted which tend to confirm this intimate relation of the latter to the overlying Devonian strata. On the eastern shore of the St. Croix River in Charlotte County, ten miles above St. Andrews, is exposed a succession of granitoid rocks, resting upon Laurentian gneiss, which both in color and texture recall the granites of the Nerepis range. They are of red or reddish gray colors, often weathering to a bright rusty red, and are, for the most part, an imperfect syenite, containing much red felspar and a soft green uncrystalline mineral allied to hornblende. Portions of the rock are highly epidotic. In descending the river these granitoid rocks are followed by, and seem to pass into, fine dark gray felspathic rocks, which are in turn succeeded by gray felspathic and epidotic sandstones. These latter, like similar beds in Perry, Maine, contain shells of Lingulæ.

On the western side of the river these red granitoid rocks form the major portion of the shore through Robbinston, in Maine. There are two objections to the view that the Nerepis granites are Devonian which may have some weight. In the slate country, between the granite ridge and the Bay of Fundy, where Upper Silurian and Siluro-Devonian strata are upturned, and show their basset edges for many miles, no granitic rocks, or mass of sediments which in bulk and texture would represent them, appear at the base of the Siluro-Devonian. Again, the movements which ensued towards the close of the Devonian age, and in the interval between the latter and the unconformable deposition of Carboniferous sediments, would seem to have found in the area now occupied by the granites a resisting mass, against which the slates were pressed up on either side. Unless then such a barrier were afforded by the Laurentian gneiss, which both in western Charlotte and at Hampstead in Queen's County, are seen to lie beneath the granites of the Nerepis, these latter would appear to have been already metamorphosed and hardened prior to the deposition of the overlying slaty deposits, or if in the condition of ordinary sandstones to have been at least more unyielding than these latter.

The Huronian formation of St. John County has many points of resemblance to the Siluro-Devonian series. A transformation of grits and conglomerates of that series into red syenite was commented on by one of the authors of this paper in a former article.* But these lack the porphyritic structure and coarse texture of the Nerepis granite, and we could find no evidence of the presence of the older series in the tract where these granites occur.

It may be noted as a significant fact in this connection, that at every point but one,† where the border of the granitic mass has been examined, Siluro-Devonian slates have been found to be *next* them, and to dip away from the granitic ridge on both sides. These slates (or petrosilicious rocks) on the south side of the ridge, belong to the Lower Division of the series, so that the granites, if they are altered sediments of the same series, are at the base of this lower division.

It may be noted also, as tending to confirm this view, that *near* the granite in the metamorphic country north of the coal-field, fossil shells have been found indicating an horizon near the base of the Devonian series or at the summit of the Silurian. Should the slates containing these fossils be found to dip *beneath* the granite, which is coarsely porphyritic, and in other features bears much resemblance to the granites of the Nerepis, little doubt of the Silurian age of these latter will then exist.‡

On the whole, the facts thus far known strongly favor the view that the Nerepis granites are altered sandstone and grits at the base of the Siluro-Devonian series.

Dadoxylon sandstone.—In St. John County a part of the

* G. F. Matthew—Quarterly Journal of Geological Society, Nov., 1865.

† In the exceptional cases referred to the granitic rocks are brought in contact with the Kingston series (Siluro-Devonian ?) by a fault and upthrow of the latter. Opposite to this point (where the granite approaches the Bay of Fundy) the lower beds in the coastal deposit of the Upper Devonian, more nearly resembles granite than elsewhere.

‡ Such would appear to be the case in Nova Scotia, where along the southern side of the Annapolis Valley, a series of slates, holding an assemblage of shells of Lower Devonian aspect, are described by Dr. Dawson as dipping downward towards a mass of coarse porphyritic granite.

Devonian strata have been called the "Dadoxylon sandstones," on account of the numerous trunks of trees of this genus imbedded in the sandy layers. The formation here also contains an abundant and varied flora of the period, as well as remains of insects and crustaceans. Within the county, and for many miles beyond it to the westward, these sandstones are intercalated with slaty beds. Immediately around the sandstone tract these slates are soft, black and carbonaceous; farther to the westward they alternate with silicious layers; and in the Nerepis Hills to the north-west, and about Passamaquoddy Bay to the south-west, the Siluro-Devonian slates, although distinctly banded with alternating gray and black layers, are silicious throughout.

Mispeck Rocks.—In passing upward, from the strata just described, to this division, a decided change may be observed. There is here a return to diorites, conglomerates and bright colored slates, such as may be seen in the lower series. These bright colored rocks are succeeded by pale green slates ("Cordaite"). In the Siluro-Devonian series they are represented by felspathic slates, fine grained homogeneous felspathic rocks, of dark gray (white weathering) and dusky red (bright red weathering) colors, which are often porphyritic with crystals of orthoclase, and become veritable orthophyres, or claystone porphyries, without any trace of lamination, except that in a few places they exhibit distinct bands of color. These colored layers are in the lower portion of the mass.

On the south side of the granitic ridge there is about the middle of this division a group of conglomerates and slate beds, differing from that at the base in the absence of dioritic and other green beds. These conglomerates are covered by a thick mass of altered grits, often granitoid in appearance, but at other times assuming the aspect of coarse talcose and felspathic schist or gritty felsites.

Some impure earthy limestone beds, of no great thickness, with talcose and chloritic slates, are found at the summit of the series.

Conditions of deposition.—It may be inferred, from the nature of the deposits in the Upper Devonian series, that by far the greater part was of littoral origin, or deposited in shallow basins partly shut off from the sea.*

From the Devonian and Siluro-Devonian rocks, to which the above remarks are limited, the slates on the north side of the granite ridge differ in their remarkable uniformity over large areas — a uniformity so great that exposures seventy or eighty miles apart present strata of exactly the same character. In this respect they are more nearly in accord with the Kingston rocks, south of the granite. Although dipping off, therefore, from the latter on the north as the Siluro-Devonian slates do on the south, and though they present a succession of beds parallel to these in general aspect, we do not as yet feel confident in asserting that they are the same.

Granting the Devonian age of these rocks, the relation of the areas north-west and south-east of the granite ridge would appear to have been, as regards the conditions of deposition, very much the same at this time as during the Carboniferous era. In the open basin or pelagic area to the north, the strata present the following succession:

Felspathic eurites, } porphyritic.
Coarse granites, }
Felsites.
1. Dark clay slate or carbonaceous schist.
2. Micaceous quartzite or quartz on mica schist.
3. Fine greenish micaceous slate.
4. Coarse green micaceous slate.

In the slates of No. 1, north of the Douglas Valley in Queen's County, plants which resemble those of St. John have been found. About twelve miles farther east numerous shells, crinoids and some trilobites occur in nearly horizontal beds of gray shale underlying dark gray silicious felsites. To the west fine grained ferruginous gneiss, micaceous quartzite, and mica slates occur in connection with this same division. In these mica slates well defined crystals of *staurotide*, *andalusite* and *garnet* have been developed.

Nos. 3 and 4 are a succession of highly micaceous slates, pale green and argillaceous in the lower part, but of a brighter apple green tint and of a coarser texture in the upper.

Disturbances at the close of the Devonian age.—In the com-

*The occurrence of land plants in the Lepreau Division on the St. Croix and Nerepis as well as at St. John, together with the occurrence of *Cordaites* through the entire mass of the Lower Mispeck rocks, near the city last named, leave little doubt of the correctness of this view.

position and structure of several of the groups to which the preceding remarks refer, many features present themselves which suggest the latter portion of the Devonian Period, or the interval which elapsed between this and the opening of the Carboniferous era, as having been in this part of the continent one of marked physical changes. Great alterations of level, marked by excessive denudation, had no doubt taken place in earlier periods, as at the close of the Laurentian, and towards or at the close of the Lower Silurian Period, but as the Devonian age was drawing to a close movements of greater magnitude, and involving all the formations of earlier date, would seem to have taken place in this portion of Acadia.

The features to which reference has been made, consist in the metamorphism and debituminization of the sediments, as well as in the extreme plications of the strata, accompanied by the production of a slaty cleavage in the more schistose beds. In these plications the Devonian beds have been affected equally with those of the Cambrian and Primordial, while on the denuded edges of both the Lower Carboniferous sediments rest unconformably. The first indications of these changes which we have are to be found in the passage beds between the Lepreau and Mispeck divisions of the Devonian series. A rapid decrease in the dip of the slates at this horizon, observed at several points in the littoral zone south of the granite range, probably marks the beginning of the great displacements which culminated at the close of this age.

It has already been said that the great gneiss-granite range in the centre of the southern metamorphic belt continued to be a stable area against which the slates were pushed up. This is very clearly seen on its south-east side, where a law of displacement, similar to that traced out by Rogers in the Alleghanies, and by Sir W. E. Logan among the rocks of the Quebec Group, holds. Instances of it may be seen in the eastern part of St. John County, where the lower Cambrian slates are met with on the south-east side of a fold, and the higher beds of the same formation with primordial slates on the other. In the western part of the same county similar displacements may be seen, having the strata uplifted on the south-east side. But the most strongly marked break is one nearer to the granite ridge; this fault runs parallel to it for forty miles, with the Siluro-Devonian slates on the north-west side, and slates of the Kingston series on the south-east.

The same granitic axis has also played an important part in influencing the direction of cleavage planes in the slates. These on both sides of the ridge are parallel to its general course, and dip away from it on each side; even where the course of the slates changes, the direction of the slaty cleavage remains the same, so that masses of the Devonian rocks are sometimes seen in which the cleavage planes cross the strike of the beds at right angles.

THE SILURO-DEVONIAN IN MAINE.

Having found, contrary to expectation, that the rocks in the greater part of the metamorphic country in New Brunswick, near the United States border, are of Devonian age, and since the various bands of slate on the British side have been traced through portions of Maine, by Professor C. H. Hitchcock and others connected with the survey of that state, we venture to offer here some suggestions and conjectures on the probable age of the schists, granites, etc., in the south-eastern half of Maine.

The granite ridge of southern New Brunswick, to which allusion has frequently been made in preceding pages, enters the State of Maine in the township of Calais. It is here represented by a thick body of conglomerate-gneiss (composed of dark syenitic pebbles, from two inches to as many feet in diameter, enclosed in a white granitic, often porphyroid, matrix), dark syenitic gneiss and white granite, which we believe to be Laurentian, and a mass of red weathering coarse granitoid rocks which may represent those of the Nerepis, and perhaps constitute the basal portion of the Siluro-Devonian. Both of these are probably represented in the granitic district of south-eastern Maine, which, according to Professor Hitchcock, is continuous to the sea in the vicinity of Jonesport.

To the eastward of this ridge we appear to have chiefly Siluro-Devonian rocks, with occasional bands of upturned Upper Silurian.. The "traps" of this area correspond to the diorites, etc., at the base of the upper division, and the "red jasper" to the red felsites and orthophyre above them. It is probable that the lower division will be but meagrely represented, and the upper half of the upper division wanting in this tract, such being the case around Passamaquoddy Bay.

On the north-west side of the granite ridge noted, we again meet, in New Brunswick, Devonian slates, now in their pelagic aspect. On the Maine border, above Baring, these consist of fine-grained gray gneiss and micaceous quartzite, the former dipping towards and abruptly meeting the gneiss conglomerate above alluded to, within which along the line of junction small pieces of the Devonian gneiss are imbedded, as though fragments of the latter had sunk in the pasty mass.* Farther north these Devonian beds are folded and dip northward, passing beneath a heavy body of fine greenish and grayish micaceous slate, which here represent, perhaps, some portion of the Siluro-Devonian series.

A similar arrangement is indicated by Professor Hitchcock, who represents the slates or schists north of Baring as lying in a basin between the granitic ridge above named, and another which crosses the northern part of Washington County, and is supposed to connect through the northern part of Hancock County with the granitic masses around Mount Desert on the coast. On the southern side of this last granitic ridge, and forming the northern side of the trough are a series of beds, described as quartz-rock and calciferous mica schist, and which are said to be the same as those known to extend through York County, N. B., towards the Bay de Chaleur. This belt of rocks has been recognized, with essentially the same features, by one of the authors of this paper, on the St. John River above Fredericton, and about Grand Lake in the eastern Schoodic region, in the State of Maine. As observed by the latter, it consists of clay slates† and thick intercalated beds of quartzite, etc., rather than of mica schist, and if, as may be

* Dr. Dawson, in his "Acadian Geology" (2d ed., p. 499), describes a similar occurrence in the case of the Devonian rocks on the south side of the Annapolis Valley. These slates, holding fossils of Lower Devonian aspect, are described as dipping into a great mass of white granite, the slates near the junction having been turned into gneissoid rock holding garnets, while numerous angular fragments are enclosed in the granite, which also sends veins into the slates.

† It is in this band of slates that fossils of Devonian aspect have been obtained by Mr. C. Robb, north-east of Fredericton.

the case, it here represents the rocks of the Lower Devonian series, it more nearly resemble these as seen near St. John than those above alluded to as forming the southern side of the trough now under consideration.

The granites on the north side of this basin, both in New Brunswick and Maine, are coarsely porphyritic. They have recently been examined on the St. John River, where along their southern border they are of a reddish tint, containing both orthoclase and albite or oligoclase. Farther north the rock is of a lighter color, consisting of a gray granite (with white orthoclase and black mica), and sometimes contains masses of dark micaceous quartzite. At several points it is overlaid by gray gneiss, holding bands of micaceous quartzite, which also constitute the rocks first seen on the northern slope of the granitic mass. These may be the "argillo-micaceous schists," described by Professor Hitchcock as holding a similar position in Maine, and which are said to extend in an "essentially unaltered form to the Saco River," in fact nearly reaching the south-west corner of the State. At this end of the basin, where probably the lower beds are exposed, the rock contains garnets, *staurotide* and *kyanite*. Along the north-east side (in Northport) it holds *andalusite*. If these rocks represent here the lower part of the Devonian slates, as the mica schists holding a similar position and containing the same minerals do in the central parts of Charlotte County, the geology of this portion of America will be greatly simplified.

There is a belt of granite associated with masses of obscurely stratified gneiss and beds of pyritiferous mica-schist, extending along the coast of Maine, from Portland eastward to the mouth of the Penobscot River, which, as described in Professor Hitchcock's Report, resembles the Laurentian series of New Brunswick. With this exception and possibly that of the belt of slates and quartzites* which skirt the southern edge of the northern granite belt, nearly all the formations of south-eastern Maine might, on lithological grounds, be com-

*A boulder composed of rock, not distinguishable from these quartzites, has been found by Mr. Charles Robb, near the Eel River in York County, which contains several unmistakeable fragments of graptolites. Should these be found to characterize this belt, the latter would of course be referred to the Lower Silurian, which as seen in St. John, they resemble quite as closely as they do the Devonian.

pared with those of the Siluro-Devonian and Devonian series in New Brunswick. Among these, however, may be islands or ridges of older rock, as is probably the case at some points along the eastern border.

One object in preparing these remarks has been partly to enable New England geologists to test the value of our work in New Brunswick, bearing on the geology of Maine. As it may assist in the discovery of organic remains in the Devonian rocks of that State, we may add that with us the Mispeck division is almost devoid of such, so far as we know, except to the eastward of St. John, where plant remains occur sparingly in the lower half ("cordaite shales"). The Lepreau division is the great repository both for these and for marine organisms. Plants are more abundant in the upper and more silicious half (equivalent to the Dadoxylon sandstone), and shells with a few plants in the lower.

It may be inferred that the discovery of such remains in the metamorphic tracts of New England is not improbable, from the fact that they are met with in New Brunswick, only a few miles from points where the slates are so highly altered as to be filled with crystals of andalusite, staurotide and garnet.

NOTE.

It is proper to add here that the foregoing article will be found to differ in several particulars from that presented and read under the same title, by one of the authors, at the meeting in Salem. The alterations referred to have been deemed necessary from the result of further investigation of the fossils upon which some of the conclusions therein given were based. At the date of the preparation of the original article these fossils were regarded by a competent authority as probably Upper Devonian, and this age was accordingly assigned to the whole of the associated strata. Having, however, since discovered, at the base of the strata thus designated, beds which contain forms apparently of Upper Silurian type, we have given this assemblage of beds the more comprehensive title of "Siluro-Devonian," intending thereby to indicate a geological horizon near the junction of these two formations, and including therefore both Upper Silurian and Lower Devonian forms of life.

It will follow from the same fact, as indicated above, that the Nerepis granites, before looked upon as probably Lower Devonian, must now be regarded as of Upper Silurian age, if not of still greater antiquity.

Lists of the fossils referred to above, with more detailed descriptions of the formations in which they occur, will shortly appear in the Reports of the Geological Survey of Canada.—*L. W. Bailey, April*, 1870.

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3. On the Valley of the Amazon. By James Orton, of Poughkeepsie, N. Y.

From the Atlantic shore to the foot of the Andes, and from the Orinoco to the Paraguay, stretches the great valley of the Amazon. Its area, of two millions and a half square miles, would contain the basins of the Mississippi, the Danube, the Nile and the Hoang-Ho. It lies between three grand elevations: on the north are the highlands of Guiana; on the south rise the table-lands of Matto-Grosso; on the west stand the Andes. The valley begins at such an altitude that on the westernmost edge vegetation differs as much from the vegetation at Para, though in the same latitude, as the flora of Canada from the flora of the West Indies. The greater part, however, is an extensive plain very slightly inclined towards the Atlantic.

From the mouth of the Napo to the ocean, a distance of 1800 miles in a straight line, the slope is one foot in five miles. Professor Agassiz gives the average slope as hardly more than a foot in ten miles; but this is based on the farther assertion that the distance from Tabatinga to the seashore is more than 2000 miles in a straight line. It is not 1600. At Coca, on the Napo, the altitude is 850 feet, according to my own observations; at Tingo Maria on the Huallaga, it is 2200 feet, according to Herndon; at the junction of the Negro with the Cassiquiari it is 400 feet, according to Wallace, and at the mouth of the Marmoré it is 800 feet, according to Gibbon;

while at the Pongo de Manseriche, where the Amazon leaps from the Andes for the last time, the altitude is 1600 feet, according to Humboldt.

These barometrical measurements represent the basin of the Great River as a trough lying parallel to the Equator, the south side having double the inclination of the northern, and the whole narrowing and gently sloping eastward. Furthermore, the channel of the Amazon is not in the centre of this basin, but lies to the north of it. Thus the hills of Almeyrim rise directly from the river, while the first falls on the Tocantins, Xingu and Tapajos occur nearly 200 miles above their mouths. The rapids of San Gabriel on the Negro are 175 miles from the Amazon, while the first obstruction to the navigation of the Madeira are 100 miles farther from the Great River.

No region on the globe of equal extent has such a monotonous geology. On the north is the low, level water-shed between the Amazon and Orinoco, composed of granite and gneiss slightly covered with debris; there is a total absence of sedimentary rocks.* On the south is the high plateau of Brazil, consisting of horizontal strata of palæozoic age, nowhere covered by secondary or tertiary deposits.† On the west are the porphyritic peaks of the Andes. Around the rim of the basin are the out-croppings of a cretaceous deposit; this is the first chapter in its known geologic history. But above this, lining the whole valley from New Granada to the Argentine Republic, are the following formations:

First, a stratified accumulation of sand; second, a series of laminated clays of divers colors and generally without a pebble; third, a fine, compact sandstone; fourth, a course, porous sandstone, highly ferruginous; and finally, over the undulating surface of the last, there was left an ochraceous, unstratified sandy clay, resembling in composition the inundation mud of the Rhine and Nile. The total thickness of these beds cannot be less than 1000 feet. The ferruginous sandstone alone is over 800 feet thick; but the table topped hills of Almeyrim are almost the sole relics. If the plausible theory be true that these are the mementoes of a colossal denudation, the history of the Amazonian Valley is quite different from that

* Evan Hopkins, F. G. S.
† Dr. Lund.

of the Pampas where there is no evidence of much superficial denudation. The trend of these hills, east and west, would indicate that the denuding force came from the Andes.

It is a question to what period this vast accumulation is to be assigned. The earlier observers pronounced it of marine origin, Humboldt calling it Old Red Sandstone, and Martius, New Red. It can be neither of these, for it overlies the cretaceous. Professor Agassiz gives it a post-tertiary date and fresh-water origin. It is "drift (he says), the glacial deposit brought down from the Andes and worked over by the melting of the ice which transported it." The Professor farther declares that these deposits "show no sign whatever of a marine origin; no sea-shells nor remains of any marine animal have as yet been found throughout their whole extent; tertiary deposits have never been observed in any part of the Amazonian basin." In the words of Mr. Lyell: "Professor Agassiz has hazarded the startling conjecture that the Amazonian basin was closed up and converted into a lake by the terminal moraine of a glacier which stretched for thousands of miles from west to east and entered the sea under the equator. But this distinguished naturalist, Lyell continues, candidly confesses that he failed to discover any of those proofs which we are accustomed to regard, even in temperate latitudes, as essential for the establishment of the former existence of glaciers where they are now no more. No glaciated pebbles, or far transported angular blocks with polished and striated sides, no extensive surface of rock, smooth and traversed by rectilinear furrows, were observed" *

It is true that neither Bates, Wallace nor Agassiz found any marine fossils on the banks of the Great River. But these explorers ascended no farther than Tabatinga. Two hundred miles west of that fort is the little village of Pebas at the confluence of the Ambiyacu. In December, 1867, it was my fortune, in coming down from the Andes by the Rio Napo and Maranon, to stop at this place. In the high bank on which the village stands, I discovered a fossiliferous bed interstratified with the variegated clays so peculiar to the Amazon. It was crowded with marine, or at least brackish-water shells! They belonged to the genera *Neritina*, *Turbonilla*, *Mesalia*, *Tellina*

* Lyell's *Principles*, I, 468-9.

and the new genus *Pachydon* resembling Isocardia. They were all new species, excepting a *Neritina pupa*, which, by the retention of its peculiar markings and by its being a living West Indian species, points to a recent era. I may add, that a sample of the red clay from a different locality, was examined under the microscope at my request by Professor Clark, who reported "fragments of gasteropod shell and bivalve casts." Moreover, mingled with the clay deposit along the river are seams of a highly bituminous lignite. I traced it from near the mouth of the Curary on the Napo to Loreto on the Maranon, a distance of about 400 miles. It also occurs at Iquitos, where it is used as fuel.*

From these facts, I infer: first, that the Amazonian clay formation cannot be referred to the ice-period, but is late tertiary, and like the Pampean mud may be an estuary deposit; second, that we have some grounds for the supposition that not many ages ago there was a connection between the Caribbean Sea and the Upper Amazon; in other words, that Guiana has only very lately ceased to be an island. Corroborative of this is the fact that there is no mountain range on the water-shed between the Orinoco and the Negro and Japura, but the three rivers are joined by natural canals; third, that in tertiary times, a shallow sea separated into islands Guiana, Brazil and the Andes, and that the mediterranean portion, where now lies the Amazonian basin, was rendered brackish by the influx of fresh-water from these highlands; that the moment the slight elevation took place between Guiana and the Andes, and Brazil and the Andes, the accumulating floods were turned eastward and ploughed a deep channel which is now called the Amazon. The fine laminated beds show that they were deposited in quiet waters which became turbulent as they became shallow as indicated by the coarse sandstone on the top of the series.

But "it is contrary to all our knowledge of geological deposits (says Professor Agassiz) to suppose that an ocean basin of this size, which must have been submerged during an immensely long period, in order to accumulate formations of such

* Mr. Huxwell has since found many more fossil shells in the south banks of the Maranon, thirty miles below Pebas, and the natives say they occur also at Ornaguas and up the Ambiyacu.

a thickness, should not contain numerous remains of the animals formerly inhabiting it." To this objection I reply that the paucity of shells is as remarkable in the similar deposit on the Rio Plata, and farther, that negative evidence is no evidence at all. To quote the language of Mr. Darwin, the discovery of the Pebas shells "is one more most striking instance how rash it is to assert that any deposit is not a marine formation because it does not contain fossils."

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4. The Plasticity of Pebbles and Rocks. By William P. Blake, of San Francisco, Cal.

At the Newport meeting of this Association in 1860, the attention of the members was directed by Mr. Charles H. Hitchcock to the peculiar elongated structure of the conglomerate at Purgatory.* In that communication and in a subsequent elaborate paper by the late Professor Hitchcock, published in the "American Journal of Science,"† it was maintained that the pebbles composing the Newport and other conglomerates had been elongated, compressed and distorted by tension and pressure after having been rendered plastic by an elevation of temperature.

Objections were made at the Newport meeting to this view of the origin of the structure, one eminent geologist and physicist, Professor Rogers, arguing that these pebbles had not been drawn out, that their original forms as deposited had not been changed, but that their peculiar elongated forms were due entirely to their having been moulded by wave action out of oblong fragments of the original metamorphic rocks.

At subsequent meetings the subject has been more fully discussed, and there yet appears to be considerable difference in opinion among geologists, upon the origin of this peculiar elongated and flattened structure. Other localities have been

* "Geology of the Island of Aquidneck," Proc. Amer. Assoc., xiv, 1860.
† Amer. Journ. Science [2], xxxi, 372, May, 1861.

noticed in Vermont* and in Maine,† and I now present some fresh evidence from the distant regions of Arizona and California upon this interesting question.

In Arizona Territory, near La Paz upon the Colorado River, there are extensive outcrops of a conglomerate made up of a paste of micaceous schist filled with pebbles of granular quartz, varying in size from an inch, or less, in diameter to masses weighing many pounds.

These pebbles, in general, present phenomena of elongation and compression similar to those of the Newport conglomerate. They give even more conclusive evidence of having been drawn out and compressed.

Elongated forms, with flattened drawn-out ends, blending at times with the mica schist are most common. The pebbles generally separate easily from the matrix and the ground is covered with those that have been detached by weathering, and which are now mingling with the modern alluvial drift. All these pebbles are uniform in texture and appear to have originally been much water-worn and well rounded by attrition. Some of the pebbles show that they have been broken across in several places, in different directions, and that the fragments have been reunited or reconsolidated as strong as before.

I was formerly skeptical in regard to the asserted distortion and plasticity of the Newport pebbles, and favored the explanation that the elongated forms were produced by wave-action, but the examination of the Arizona conglomorate convinces me that not only it, but the Newport conglomerates, and those of many other localities, have been distorted and drawn out and compressed. I am sure that the examination of the outcrops would satisfy even the most skeptical.

But the evidence of distortion of hard rocks on the Pacific Coast does not rest with the Arizona conglomerates only, it is found on a large scale upon the flanks of the Sierra Nevada of California. Those who have ascended the lower slopes of the range in the gold region are familiar with the remarkable out-

* By Professor Hitchcock. See Final Report upon the Geology of Vermont.

† By Professor Charles H. Hitchcock. Preliminary Report upon the Geology of Maine, 1861. The distortion of rigid pebbles appears to have been noticed by Professor Edward Hitchcock, as early as 1833. See Report upon the Geology of Massachusetts.

crops of slates well described by the name of "gravestone slates," given to them by the miners from their resemblance to gravestones. They stand above the earth in long lines, like tall tomb-stones, and are sometimes ten or fifteen feet high, and are not over three or four feet broad at the base.

These slates vary in composition; some are like roofing slate, others are arenaceous, and some are semi-metamorphosed conglomerates with small pebbles. They are principally of the secondary period.

An examination of these remarkable slates shows that their peculiar form is due to the elongation of the grains which compose them, and consequently of the whole mass. The conglomerates show the elongation with the greatest distinctness. In some outcrops pebbles appear to have been stretched as much as twice or three times their original length or diameter. They are not only drawn out but flattened so as to become long lenticular masses, thus giving a slaty structure to a rock originally made of rounded pebbles. Examples might be multiplied almost indefinitely. Vast masses of rock have been thus acted on, and this drawing out and elongation of mountain masses of rock is more common than has been generally supposed.

All these phenomena indicate that the flexure or folding of rocky strata on a large scale must give rise to great tension upon the outer curve of the bend. Professor Hitchcock supposes the tension by which the rocks were elongated to have been produced in this way in some cases. He remarks also, that the Vermont rocks appear to be stretched in the direction of the dip, while at Newport they are elongated horizontally. Nearly all the examples in California show the elongation to be in the direction of the dip. But I believe the rocks to have been subjected to a much greater elongation than can have been given by any folding. I regard them as having been subjected to direct tension over large areas, and generally in vertical or highly inclined planes. Moreover, these elongated masses do not appear in such positions that we can regard them (at least in most cases) as forming portions of great anticlinal arches. They may form the sides of great synclinal troughs and have been under great tension during subsidence of a mass of formations in the centre of the trough.

It may here be observed that this great elongation of rock masses, and the flattening of all the grains of sand and of pebbles which compose them (an elongation in some cases to twice or three times their original length), has been accomplished at the expense of their thickness. Thus strata so elongated are much thinner than in their unstretched condition. This is a consideration which bears directly upon the discussion of the probable height of anticlinal folds.

With regard to the condition of the quartz pebbles, and of the rocks during the process of elongation, there is room for wide speculation and a variety of hypotheses. Scrope, Beaumont, Scheerer, Hunt and others, maintain that all the deep-seated rocks become plastic. We cannot, of course, easily conceive how this distortion of the hard pebbles could have been effected when in their ordinary condition. That rocks are much softer in the bed or quarry than after they have been raised and exposed to the air, is a familiar fact to all miners and quarrymen. This softness of rocks may perhaps be, and probably is, increased by an elevation of temperature. We may legitimately invoke the agency of heat and water to aid us in accounting for these interesting phenomena, but I conceive that it is not necessary for us to believe that these changes of form were effected at very elevated temperatures. There does not appear to have been anything like semifusion or viscidity of the mass, and when I use the term *plasticity* I do not connect with it the idea of any great softening produced by heat. The consideration of the phenomena leads me rather to the conclusion that enormous and long continued pressure and tension, at a moderate elevation of temperature, perhaps (but not necessarily so), have been sufficient to produced the molecular movement of these hard and apparently unyielding materials. Water permeating the mass, or the vapor of water, may faciliate this movement, but there does not appear to me to have been any condition involving a great chemical change. The evidences of such changes are wanting. Mechanical force alone appears to have been the agent. This I not only consider to have been the cause of the distortion of pebbles and rocks, but to have been sufficient to reunite fragments of pebbles or rocks so as to make them homogeneous. This may be by some considered as an example of cementation by solutions—a kind of rock regelation—which certainly might occur and probably does, but we are not precluded from the conception of the possibility of the fragments being reunited simply by pressure, when under favorable conditions. I may here refer, in support of this view, to the beautiful experiments made by Mr. Hungerford at the Chicago meeting, in reuniting the fragments of ice when at such a low temperature as to preclude the idea of there being any fusion of the contiguous surfaces of the fragments. Examples of the mobility of the particles of metals at our ordinary temperatures are numerous and familiar.

Lead at a temperature below fusion is forced by hydraulic pressure into pipe; every coin and medal has been moulded by pressure, and iron may be forged or drawn into wire either hot or cold. Tersca* has shown that under enormous pressures solids can be made to flow in the same manner as liquids or that in their movements they follow the same law. If a strong cylindrical mould be taken, open at one end and partly closed at the other, discs of iron placed in it may be forced out of the small opening by powerful pressure from a follower or piston in the cylinder, and these discs are changed into cylinders or a mass of elongated cones. It may here be observed, incidentally, that in this we appear to have a direct illustration of the mode of formation of the curious forms found in rocks called stylolites, and of those generally known as "cone within cone," described by Professor Marsh at the Burlington meeting, the former of which was regarded by him as the result of pressure, and the latter of pressure on concretionary structure when in process of formation, while the rocks were soft or in a plastic state. The distortion of fossils is another familiar example of rock plasticity. Tyndall, in the appendix to his work upon the "Glaciers of the Alps," expresses the opinion that a mass of solid glass may, by pressure, be forced to permanently change its form, and that some rigid pebbles of

* Mémoire sur l'éconlement des corps solides soumis à de fortes pressions par M. H. Tresca, Compte Rendus, T. LIX, 1864, p. 754.

quartz, in the Museum of the Government School of Mines, have been squeezed by enormous pressure against each other so as to produce mutual flattening and indentation. We have also an example in the columns of the House of Representatives, quarried on the banks of the Potomac, where the calcareous pebbles appear to have been forced by pressure one into the other. Professor Ramsay described the pebbles in the Museum of the Government School of Mines, as from three to nine inches in diameter, and he thought that the indentations were produced by wearing or the rubbing of one pebble against another while under great pressure, and perhaps partly by the aid of intervening grains of sand. Mr. Sorby regards the interpenetration or impressment of pebbles as due to mechanical and chemical agencies combined, and cites the fact that in the majority of substances mechanical pressure increases their solubility.

Such explanations are not satisfactory for the Arizona conglomerate. Mechanical pressure and tension alone appears to me to have accomplished the result. This also appears to me to be the most satisfactory explanation of all other examples that I have seen. There does not appear to have been any solution, or at any rate not sufficient to affect the form and surface of the original pebbles. The Arizona pebbles are almost as clean and smooth as if just out of the bed of the brook. They separate readily from the lamellar part of the rock. They are not cemented to it as would probably be the case if they had either been softened by great heat or partly dissolved or acted upon chemically. So also in the case of the large masses of rock, the outlines of the small pebbles do not become obliterated although the form is so much changed that they can hardly be recognized as having been originally in the form of pebbles.

In these phenomena we see how a rock which was originally granular and made up of pebbles may become entirely changed in structure. Deposited as a conglomerate it may become a lamellar slaty mass extended in one direction to twice its former linear dimensions. It will split up or cleave easily in one direction and not in another, and will weather unequally as we find in the sharp outcrops of gravestone slates of

California. The same elongating and compressing forces applied to finer sediments must of necessity greatly modify their structure and cleavage, but the change of texture, owing to the fineness of the particles, does not become visible to the eye. By the careful study of these phenomena of plasticity new views are opened to us of the structure of great rock-masses; of the phenomena of plication, of lamination and of the origin of some structural peculiarities of mineral veins and their inclosing walls. In view of all the facts I think that geologists should be more willing to admit that very great changes have been produced in the structure of rocks and rock-masses by simple mechanical force unaided by any great elevation of temperature or by extraordinary chemical agencies.

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5. On Some Recent Geological Changes in North-eastern Wisconsin. By G. R. Stuntz of Lancaster, Wis.

[Communicated orginally, March 12, 1854.]

This is the second season I have spent in this new and but partially explored region. In the summer of 1852, I arrived on this part of the lake, with a party of twelve men, for the purpose of extending the Surveys of the United States bordering upon the south-west coast: and, also, of running the boundary line between the State of Wisconsin and the territory of Minnesota. Since the completion of those works I have been voyaging in all directions through this country and between the lake and the Mississippi River. The peculiarities of this lake are fully described by Foster, Whitney and Dr. Owen, in reference to tides, in their able geological reports; also to a change of water-level in the lake. I would here state that since my arrival at the mouth of the St. Louis River, in July 1852, the water of the lake had fallen, and was on the first of November, 1853, twenty inches lower. This is probably the periodical change of level. There is another change of level apparent, which I do not recollect having seen noticed in any report. That is the gradual rise of water at this end of the lake, and the falling of the same at the east.

The mill race at the falls of the Ste. Marie River, only a few years since, was used by good sized Mackinac boats as a canal in making the portage around those dangerous Rapids. In the summers of 1852 and 1853, this mill race was entirely dry, which fact is referred to by J. W. Foster in his able report. The wearing away of the channel, upon a hasty view of the subject, would be the natural conclusion, but the facts do not warrant it. Should the tributaries that pour an unceasing tide be cut off, except enough to counteract evaporation, and should the river Ste. Marie continue to discharge the same volume as at present, which is about one billion, eight hundred and forty-eight million, three hundred and twelve thousand cubic yards per year; and should Lake Superior be allowed to contain thirty-two thousand square miles, it would take over fifty-three and a half years for the lake to fall one yard, which is about the original depth of the mill race before referred to; and this is a much longer period of time than has transpired since that race was used for the purposes before mentioned.

The small stream at Pindell's mill, a few miles above Jaquois Point, runs with a rapid current to the lake, having no marshes, and not widening nor giving any indications that its valley is overflowing by the lake setting back into it, but on the contrary the formation of sand about the mouth indicates a gradual receding of the waters of the lake. As you go westward, the Ontonagon River exhibits a slight filling up. The valley near the mouth shows that at the time it was excavated the surface of the lake was lower than at present. The same is also apparent at the mouth of Bad River still farther west.

At the mouth of Bois Brulé the same thing is exhibited, only to a greater extent. From this to the west end of the lake not only does the lake set back into the valleys of the streams, but the waters are making rapid encroachments on the banks. So rapidly is the filling back, that the deposits of the streams do not keep pace with the filling up. The consequence is, that there is a large marsh and pond in the mouth of the valley of Bois Brulé and Aminecan River. But nowhere is this filling up more apparent than in the bay above the mouth of the St. Louis River. In several parts submerged stumps, several feet below the present water level, are found. The numerous inlets surrounding the main bay, when we consider the nature of the soil and the formation (a tough red clay), in all of which the water is deep, could not have been excavated in the natural course of events with the water at its present level. The testimony of the Indians also goes to strengthen the same conclusions. At the time of running the state line above mentioned, the Indians, ever jealous of their rights, called me to a council to inquire why I run the line through Indian land. In the explanation, I gave, using the language of the law as a starting point, the lowest rapid in the St. Louis River. The chief immediately replied, that formerly there was a rapid nearly opposite the Indian village. Start, said he, from that place and you will be near the treaty line. After he had been farther questioned I learned that it was only a few years since the river was quite rapid at the Indian village. At the time the said line was run the first rapid was about one mile by the stream above the village. From these facts, I conclude that a change is taking place gradually in the level of this great valley. This change may lead in time to as important results in changing the geography of the country as have taken place within a comparatively short period of time in the valley of the St. Croix. The St. Croix Valley is about forty miles in diameter north and south, and about one hundred, or one hundred and twenty miles, east and west, surrounded, or nearly so, by ranges of trap hills, which attain nearly a uniform height of six hundred feet. These time enduring barriers withstood the warring of the elements until within a few centuries past, when some change, probably not unlike the above, assisted them in breaking away at the falls of that river, discharging a volume of water through that narrow gorge, which in its native serenity, crystal purity, and quantity, would vie with any of the great sisterhood of lakes. But it has gone foaming and tumbling to the Gulf of Mexico, and we can only read traces of its onward flight in the numerous terraces along the river banks below, and the excavation of Lake St. Croix and Lake Pepin, and the piling of terraces along the father of waters as low down as the mouth of the Missouri.

In the valley above the falls you find, what you would expect to find, after the lapse of a few hundred years should any convulsion of nature change the valley of any of the great lakes to dry land, extensive sand planes, slightly timbered with stunted pines. There are exceptions, it is true, but the line of demarkation is as plain as though the event of draining took place only last year. What is called Wood Lake timber, is a narrow belt of elevated timber land, extending from the south-east side of the valley towards and nearly to the mouth of Kettle River. This ridge, having a good soil of clay and loam resting on a sub-soil of sand and gravel, from its elevation must have been drained at a much earlier day than the balance of the valley.

On all the streams, so far as I have examined, these appearances are the same. Standing at a point a few miles to the north-east of the mouth of Wood River, you are near the centre of a system of rivers coming from all the points of the compass, except the great outlet on the south, Upper St. Croix on the north-east, Namekagon on the east, Yellow River, Clam River and Wood River from the south-east, the first of these last three flowing through a chain of lakes of some twenty miles in extent, containing white fish in abundance. On the south-west come in Sunrise River and one or two smaller streams, from the west Snake River, and from the north-west and north Kettle River and several smaller streams. These streams; flowing from the high rocky barrier before spoken of, present a variety of scenery and a variety of soil alike interesting and valuable.

In connection, it might not be out of place to remark that other facts present themselves to the mind of the student of nature, intimately connected with the above..

Throughout the great valley of the Mississippi and bordering upon many of the great lakes are found mounds and tumuli, evidently the work of men's hands:—the labor of a race long since passed to oblivion, except as yet this single trace of their existence. That they were a uumerous race these mounds plainly indicate. That they were skilled to some extent in the arts, their mining operations at Ontonagon on this lake, near the Minnesota Mining Co.'s works, plainly prove. That they were the same people that inhabited the

Mississippi Valley, the various implements would also indicate. I have in my possession a stone hammer which I found at the ancient copper diggings near Ontonagon, and I have seen similar ones obtained in the vicinity of ancient mounds at Rock Island, Illinois.

This people, whoever they were, occupied or built their mounds, for whatever purpose they were erected, upon the finest sites the country afforded. You will find them on the finest situations for building along the great rivers, and on elevated localities commanding the finest views of the surrounding country. This is particularly remarked by every traveller on the Upper Mississippi, and the St. Croix below the falls.

As you ascend this last mentioned stream, a short distance above Moriere, you find several unusually large mounds in a small plain, elevated some sixty, perhaps one hundred feet, above the river. Thence following the river you pass the rocky gorge or Dalles and enter the valley of St. Croix above the falls. The same beautiful river rolls at your feet, and in going up a succession of banks, grassy slopes greet your sight; but the mounds are not there, and, for the distance of seventy-five miles by the stream, I have looked in vain for these relics of that obliterated people.

At the mouth of Yellow River they are again found, occupying, as before, the most beautiful sites the locality affords, and at an elevation nearly equal to the southern rim of the basin. From this point nearly in a straight line and lying intermediate with the falls of St. Croix, are situated Yellow Lake, Clam Lake and Wood Lake, all occupying elevations nearly equal to the rocky barrier, over which the waters were formerly discharged at the falls before mentioned. At each of the two last mentioned lakes a single mound of the same description occupies a prominent place in the delightful scenery of the lake. At Yellow Lake, the most attractive of the three, they are found in great numbers, extending down the river several miles, but in every instance occupying the highest elevations. In my examinations on two trips through this ancient lake-bed, I have failed to find any mounds on lower situations than those above described.

Then if this view should not be proved incorrect, we have one geological event coupled with the operations of this ancient people. And we cannot avoid coming to the conclusion that these mounds were built prior to the wearing away or breaking down of the falls of St. Croix, and that the trip from the Mississippi to Lake Superior was one of ease and pleasure compared with the journey of the present day through almost impenetrable forests. I hope some one better prepared to carry out and systematize a series of observations may profit by the above.

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6. The Gems of the United States. By Dr. A. C. Hamlin, of Bangor, Maine.

With the exception of the emerald, all of the gems in more or less perfection are found within the limits of the United States. The diamond has been discovered in California, among the Rocky Mountains and along the gold belt which extends from Central Alabama through the Atlantic States to Maryland. In Alabama, Georgia and the Carolinas the itacolumite, which has been regarded as the matrix of the diamond in the Urals, Brazil and Hindostan, appears in extended ledges, and even rises to the magnitude of mountains. In 1866, while exploring the auriferous regions of Alabama and Georgia, I recognized this rock in many places, especially near Gainsville, where it crops out in great ledges.

Diamonds have been found along the course of the itacolumite, especially near Gainsville and farther to the north-east, at the Glade and Horshaw gold mines. Some of these stones were of several carats weight and of fine water. One of these which had been polished in London was shown to me at Gainsville, and it is a gem of the purest water. From information obtained from the residents of these regions and from personal examination of the localities, I have but little doubt that active research with the application of skilled labor [for the diamond is not easily recognized in its rough state], will bring to light many fine stones. A splendid stone was destroyed by the stupidity of the laborers at the Horshaw mines a few years ago. A beautiful gem of 24 carats was found in 1856, near Richmond, Va.

The garnet is found in many of the States, and of sufficient purity for the purposes of the lapidary. At Fitchburg, in Massachusetts, beautiful little pyropes are found in the alluvial sands. In Delaware County, Pennsylvania, pyropes of larger size, but of less beauty of color, are also washed out of the alluvial soils. All along the Rocky Mountain slope clear garnets have been observed. In the sands of New Mexico the Indians find garnets of considerable size and equal to the best of the Syrian stones, exhibiting the crimson and violets tints of the oriental garnets. At Pike's Peak, in Colorado, garnets of less size, but of even finer tints, are washed out of the gravel beds by the gold miners. These are the finest of the species in America, and with the exception of the rubellite, they approach in color nearer the ruby than any other stone. Beautiful cinnamon garnets occur at Phippsburg and Parsonfield, Me., Warren, N. H., and in many other places in the States, but they are rarely sufficiently perfect for ornaments.

Chrysoberyl is found at Haddam, Ct., in New York, Vermont and in Maine, but few transparent crystals have yet been discovered. Spinel occurs in New York and New Jersey, but the crystals are generally opaque. Zircon is widely distributed in the States, generally massive and opaque. The finest crystals come from Buncombe County, North Carolina. The iolite, known as the sapphire d'eau of Ceylon, has been found at Haddam, Conn., and in other places in New England, but fine specimens are quite rare. It is often pleochroic, exhibiting different colors when viewed in different directions. The topaz occurs at Trumbull and Middleton, Conn., and in North Carolina, but the colors are generally very faint, and transparent specimens too small for the purposes of the lapidary. The amethyst is found in rare perfection in various parts of the United States. Oxford County, in Maine, Berlin Falls, in New Hampshire, and Bristol, R. I., furnish beautiful specimens. Fine stones are found in Delaware County, Pa., and at Kewanan Point, Lake Superior, but they are more plentiful in Georgia than in any other State. Several varieties of opal are found in the United States, but none of good quality like the precious and fine opals of Mexico.

Sapphires are found in several of the States; at the Chester emery mine in Massachusetts, in New Jersey, in Connecticut, New York, California, Pennsylvania and North Carolina, but they are generally massive and opaque. At El Dorado Bar, in Montana, however, they occur in transparent and well defined crystals of six-sided prisms and also in the amorphous form in the alluvial sand, together with native gold. Crystals of several carats weight have been picked up out of the pan, where they have settled down in consequence of their gravity during the process of washing the gravel for gold. Sapphires of almost all colors have been found there, the red, green, blue, yellow and white, and some of them are of considerable value. They resist the action of the fire and do not change color when exposed to the strongest heat of the forge. No systematic search has been made for these precious stones at this locality, although they seem to occur in abundance. The gold washings of this Bar are now abandoned.

The beryl—the subspecies of the emerald—occurs in many parts of the United States. It has been found in great perfection in the granite hills of Oxford County, in the State of Maine, and more especially in the ledge at Royalston and Fitchburg, in Massachusetts. In gash veins of quartz, occurring in granite in North and South Royalston, beautiful crystals of this gem have been discovered, some of them exhibiting the longitudinal striæ and the aberration of colors which distinguish the remarkable beryls of the Altai Mountains in Siberia. Lively sea and grass green, light and deep yellow; also blue crystals of various shades have been found at this locality. At the quarries at Fitchburg, beryl of a rich golden hue, approaching the chrysoberyl and topaz in color and hardness, and closely resembling the yellow diamond in lustre, have been blasted out. All the deposits where the beryl occurs seem to be very superficial, and at Royalston and Fitchburg the crystals appear to arise from the felspar, becoming clearer and more perfect as they penetrate the quartz. All of the crystals found during my explorations at these localities were thus connected with the felspar. This rule, however, was not observed at Mount Mica, where all the beryls were generally enveloped in quartz or albite, and unattached unless to crystals of muscovite.

Tourmalines of great perfection and variety of colors have been found at Mount Mica, in the State of Maine. This little hill — which is perhaps one of the most remarkable mineral localities on the globe, since nearly forty varieties have been found there within an area of thirty feet square — is one of northern spurs of streaked mountain in the town of Paris, in Oxford County. The mineral deposit was discovered in 1820, by Hon. E. L. Hamlin and Dr. Ezekiel Holmes when students, and whilst searching for minerals. At that time about forty crystals of tourmaline, some of which were quite perfect in form, of fine color and limpidity, were picked up on top of the ledge, or found in the earth which had accumulated around its base. Since this period the place has been visited by mineralogists from time to time and the entire deposit blasted out.

Some magnificent crystals of rare perfection of form and of beautiful colors have been found there. Some of these crystals were several ounces in weight, several inches in length and more than an inch in diameter. Some were red at one extremity and green at the other. Others were red within and green at the circumference. Superb stones of this description and arrangement of colors, but of less size, have also been discovered there, and they called forth the remark from the elder Silliman that they were incomparably fine and without a parallel in the world. Nearly all the varieties of this remarkable gem have been found at Mount Mica, — the clear light green, like those from the dolomites of St. Gothard, the pink of Elba, the white and yellow of Ceylon, the dark smoky green and blue of Brazil, the lighter blue of Sweden, and the fine red and green of Siberia. In some of the crystals the red passes into blue, and the blue into black or into green; in others the white changes into red, green or blue, exhibiting many intermediate shades. Generally the transitions and gradations of color are imperceptible as they pass from one into the other, but in some crystals the line of demarkation is well defined.

The tourmalines of Mount Mica occur in a coarse granite resting upon mica schist, and were probably deposited from above and from solution. The granite appears in layers, and the tourmaline streak penetrates across and through them without any reference to seam. Coarse and opaque crystals of several inches in diameter and nearly a foot in length, are found in the albite and masses of quartz, but all the fine and transparent prisms have been found in cavities whose walls were composed of smoky quartz and albite. In these cavities (which have been found of a size varying from few inches to several feet in length,) the tourmalines appear either loose in the disintegrated cookeite or arising from the interior walls and sometimes penetrating crystals of the matrix. A few feet below the surface the tourmaline streak disappears and the walls show quartz, albite or granite destitute of other minerals. This superficial degree of deposit is not confined to the tourmaline alone but is noticed with most of the other gems.

The sapphire, diamond and emerald are found near the surface, and this rule is well exemplified in the occurrence of the amethyst in the mines along the Rocky Mountain slope, where it vanishes at the depth of ten to twenty feet, although the quartz crystallizes in a colorless state even at the great depth of six hundred feet below the surface. It seems as though the gems required some ray of light or some effect of the atmosphere to build up their forms and perfect their hues. Although it is a common belief that tourmalines are of inferior value, they are not really so, and they should take a high rank among the first of the gems. For none exceed them in the phenomena of physical properties, or in complexity of composition, or in the vast range of their colors; and their hardness is quite equal to the emerald, which they surpass in refractive powers, whilst their dispersive energy exceeds even that of the sapphire and topaz.

There are many localities in the world where this stone is found, but fine specimens are rare, and when they are limpid and approach the emerald, the topaz and the sapphire in hue, they are sold for those gems. The red tourmaline, the rarest of all gems, when free from faults and of fine colors, according to the eminent authority of Professor Beudant of Paris, is sold at the price of the ruby, the most costly of all the gems.

The finest specimens of the tourmaline species known in the world have been taken from the now exhausted locality at Mount Mica. Some beautiful crystals of splendid color have been found in loose boulders at Hebron, Maine, but the parent ledge has not yet been discovered. There are other localities in the United States where these stones occur, but they are opaque or of very poor color and imperfect crystallization.

There is no gem, not even the sapphire, which surpasses the tourmaline in variety of color, and as Barbot has remarked, "it seems as if nature had wished to prove to man that she could imitate quite perfectly that which she had created the most perfect."

The Spaniards, under Cortez, whilst on their march to the capital of Mexico were astonished at the size and beauty of the emeralds and turquoises that decorated the persons of the chiefs who came to join them as allies or visit them as envoys from Montezuma. And after the conquest of the country they sought in vain for the mines whence these emeralds and turquoises came. The emeralds undoubtedly came from Central and South America, and were brought overland or along the coast as an article of trade, since they have not been found anywhere in the United States or Mexico.

The turquoise, the Mexicans said (in reply to the question of the Spaniards), came from the far north, but the mines were not discovered until recent times. The histories of the Spanish occupation of Mexico make no mention of these mines — at least so far as I am acquainted with them. A mine of great antiquity is situated in the Cerillos Mountains, eighteen miles from Santa Fé in New Mexico. The deposit occurs in soft trachyte, and an immense cavity has been excavated by the Indians in past times whilst searching for this gem. Within a few years the Navajo Indians have revealed the existence of a mine in the Sierra Blanca Mountains in New Mexico, but they will not allow strangers to visit it. Stones of transcendent beauty have been taken from it and handed down in the tribe from generation to generation as heirlooms.

Nothing tempts the cupidity of the Indians to dispose of these gems, and gratitude alone causes them to part with any of these treasures, which, like the mountaineers of Thibet, they regard with mystical reverence. The Navajos wear them as ear-drops, by boring them and attaching them to the ear by means of a deer sinew. Lesser stones are pierced, then strung on sinews, and worn as necklaces. Even the nobler Ute Indians, when stripping the ornaments of turquoise from the ears of the conquered Navajo, value them as sacred treasures, and refuse to part with them even for gold or silver. One of these magnificent stones was presented by the Navajo chief to Major-General Carleton, when Governor of New Mexico, and it may be taken as the type of the American turquoises, although there are larger stones in possession of the tribe. It is nearly an inch in length, one-third broad and one-fifth in depth, and equal, in purity and delicacy of tint, to the best of the Persian. Other mines have been reported as occurring along the Rocky Mountain range, but I have not been able to obtain any reliable information concerning them.

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7. Studies in Chemical Geogony.* In three parts. By Henry Wurtz, of New York.

Introductory Remarks.

These being subjects on which volumes might be written, the whole must needs be condensed, on such an occasion as the present, into a few brief notes, more or less disconnected, as I fear; and to views but partially expressed.

I have long held that the principle which should pervade and strictly govern all researches into the chemical conditions prevalent during past geological time, is to seek out the great natural chemical operations prevalent at this day, and trace them back to their necessary spring and beginnings.

It is true that actual laboratory researches into the characters and composition of the products and relics we possess of those far distant times, are as yet wofully deficient, and be-

*The following series of notes was prepared to be read at the Chicago meeting, in 1868, and the titles were there announced in the published lists; but as the author was not able to be present then, he preferred to await the Salem meeting.—H.W.

fore the day shall come when the riddles of the past shall be read, and its many mysteries unveiled, in the light of perfect theory, myriads of laborious researches, both analytical and synthetical, must be made; still I believe that enough has already been done in this field to justify some attempts at generalization. And though these be conceded to be pure hypotheses, still the history of science establishes the value of such hypotheses, as starting points, in stimulating discussion and in suggesting modes and subjects of experiment.

I. On the Prozoic Atmosphere and the Ocean of the Zoic Dawn.

It cannot be doubted that it was during the time of the deposition of the earliest sediments of the ocean, which are known to us now only in the forms of compacted and crystallized metamorphic masses, that life began, and that it had its beginnings in the waters of the ocean. The exterior Ocean of Atmosphere, though supporting no life in its own bosom at first, I believe to have been then, as now, most intimately and essentially connected, through the medium of the waters, with the life of the latter.

I shall begin by saying at once, and concisely, that my generalizations have led me inevitably to a novel conclusion, which will doubtless startle some; that Life was at the outset, has always been, and always must be, the governing *influence* in all chemical changes that have occurred and will occur, in the air, in the waters, and on the earth.

[It is not without a purpose that I here use the word *influence*. The words *force* and *power* I avoid, in laying down this principle, for reasons which I shall give at length on another occasion.]

In a previous paper I have put forth the proposition that at the Zoic Dawn the ocean must be believed to have been wholly in an oxygenated condition; that is, its constituents were at their maximum of oxidation. What then was the condition of the atmosphere corresponding to this? Before replying to this I must first propound another question, which involves an appeal to my fundamental doctrine of tracing back present changes to their beginnings. What, then, is our only known source of oxygen at present? Evidently the decomposition of carbonic acid by solar force, through the influence of plant-vitality; a never-ceasing agency which has continued to increase the proportion of oxygen and diminish that of carbonic acid in the air, since plants began to grow upon the earth, and has left its evidences on almost every leaf of the Book of Geological Ages.

Considering in connection with these facts the nature of oxygen, the greediness with which it enters into combination with other bodies, and applying the doctrine we started with, of following backward to their beginnings all such changes as we find in progress, I am led to lay down, as the primary postulate of a Zoic Theory of Chemical Geogony, the following: *the Prozoic Atmosphere contained very little or no free oxygen, probably none; but consisted essentially of carbonic acid, nitrogen and aqueous vapor.*

I anticipate, of course, a host of objections to so novel a view. One is that although plant-growth be independent thereof, yet germination requires free oxygen. Some may even maintain the assumption that plant-life must have commenced with germs.* To this speculative objection one answer is, why? Another answer, probably as speculative, however, as the objection itself; it is far from inconceivable, or even improbable that in a liquid medium containing such oxidating agents as the ferric compounds, the modification of eremacausis thought necessary to germination may have proceeded through the agency of these compounds.

Another objection may be that our present plant-life is accompanied by intermittent intervals of *absorption* of oxygen, during absence of the solar ray. But in any case the vegetative processes of the day we treat of, were in a measure independent of solar heat at least, if not of solar light also; of which the proof, even as late as the Carboniferous, is admitted. To the objection that in the earliest sediments we find little organic, or even carbonaceous matter, I reply that (apart from the graphites and sulphides, both believed organic in origin) it is easily understood that while life might withstand the ferric

*Those who will attach its due weight to the Mosaic Record in this regard, I may remind that the Creative Fiat called "the herb yielding seed, and the fruit-tree yielding fruit after his kind, whose seed is in itself," etc. The question is an ancient one, "which was first, the hen or the egg?"

compounds, dead organic matter would quickly be burned up thereby, the sulphides having indeed resulted from this very burning process. I have applied the same principle to far later times, in a paper presented to the geological section, to explain the absence of fossils in the American Red Sandstones.

We here recognize another, and by no means a minor source of oxygen, namely, from the original oceanic metallic sulphates, which are now extant as sulphides, having given up their oxygen to the atmosphere by this indirect process of reduction by dead organic matter. I may add, in this place, that there has been still another, and a quite important source of oxygen, freed through vital influence; that O which represents the proportion deficient in bituminous coals to form HO with the H. This represents of course O which has at first been merely transferred from H to C, but as the CO^2, once formed, is again decomposed and its O set free, it amounts to the same thing.

Certain other objections to my views have been offered to me from time to time in conference and correspondence. With regard to my induction that the Ocean of the Zoic Dawn contained ferric constituents, it has been suggested that these would have been poisonous to life. This is but a question of degree, that is, of the strength of the solution. I claim only that the metals now present in the rocks as sulphides were present in the Prozoic Ocean as sulphates. The solution, in any case, could not have been a concentrated one. Moreover, the notion that ferric solutions are inimical to life, particularly in its lower forms, I think is unsupported by evidence. *Ferrous* solutions are so, without doubt, but not ferric. It has been claimed that subaerial action of carbonic acid, on alkalic and earthy silicates, would introduce into the ocean enough of bicarbonates to render it neutral, or even alkaline, and to precipitate all ferric oxides and hydrates. No doubt this took place *locally*, and the iron ore beds of these rocks are thus (and I believe thus only) to be accounted for. We have thus actually a new and substantial prop to my theory. Still this is a question of *time*, and to bring the whole of the vast ocean to a neutral or alkaline condition, must have required, as I am convinced, time enough for the deposition of the whole mass of the Eozoic schists, if not of a portion of the Palæozoic. I find that I have been misapprehended, in that I have been supposed to hold, in my paper on Gold Genesis, presented to this Association at Buffalo, in 1866, that gold has been deposited from the ocean equally in all geological ages. By my views, it cannot but be that the oldest sulphides were the most highly auriferous, at least of those (if any such there be still in existence) that have been deposited from the oceanic menstruum directly. It is *Gold Concentration*, and the formation of gold veins, that I claimed has occurred throughout all time, and is even now going on; and going on by virtue of the same agencies that I was the first to discover in 1858, the alternate formation, by oxidation, from auriferous pyrites, of ferric solutions of gold, and their reduction to ferrous conditions again, by farther reactions with unaltered pyrites or with organic matter.

My farther notes on this branch of my subject must be cut very short.

From the above it will be seen that my reasonings have led me to believe that, in Prozoic times, all the oxygen of the earth which now enters into the processes and changes going on upon its surface, all the *potential* oxygen of the earth (so to speak) was locked up in combined forms and divided between the earth's hydrosphere and its aerosphere; combined chiefly with iron in the former and with carbon in the latter.

I would next present the proposition that substantially all the potential *chlorine* was contained in the Prozoic, as it is in the present Ocean; as we know of no method by which chlorine (not probably oceanic) is now being introduced into the Ocean. As to *sulphur* and *phosphorus*, it is not certain that these two were *all* in solution in the original Ocean, as the fundamental rocky substratum or nucleus of the earth's crust being even at this time unrecognized, there is no certainty that it did not contain sulphides and phosphates before its subaerial erosion began. It is clear, however, that most of the sulphur, and probably of the phosphorus, of all sedimentary rocks was in the earliest Ocean, in acid forms. This Ocean then contained not less combined acids than ours.

With regard now to the metals of the Prozoic Ocean, or the bases with which its acids were combined; ferric and other oxides of the heavy metals, as I have shown, were among these, but never, of course, formed more than a minor percentage of the whole. One consideration is salient here, which is insisted on by STERRY HUNT, and scarce admits of a doubt, that the *characteristic* constituent of our Ocean, *sea-salt*, was originally present in far smaller proportion than now, and that it has been, at least in great part, a gradual product of the reaction of other preëxistent chlorides with the carbonate of soda that is continually carried in by rivers. It is thus that the carbonate of lime of sea shells is supplied. Carbonate of potash is also thus carried in, but the potash would appear to have been eliminated again in insoluble forms, such as *glauconite;* while we know no provision for removal of the soda. What were these preëxistent chlorides? Here is a point where, in our present condition of knowledge, there can be little but speculation; and where the great field for future investigation lies. There has already been discussion as to the questions whether *chloride of aluminum* was present or not (to the affirmative of which I incline): whether the amounts of the chlorides of *calcium* and magnesium, or either of them, were much greater formerly than now (of which I should incline to maintain the negative); which of the bases of the latter two chlorides predominated in the older Ocean, and so on. STERRY HUNT at one time made the suggestion that the waters of mineral springs from Silurian rocks, largely impregnated with these two chlorides, may be the fossil waters of the Silurian Ocean; but it seems probable rather that these are bitterns from the evaporations of enclosed portions of this Ocean. C. A. GOESSMANN, in a paper in the *American Journal of Science*, July, 1867, gives us a comparison between STERRY HUNT's analysis of a salt spring* and his own of a Syracuse bittern, which has "practically ceased to evaporate" in the open air.

	(GOESSMANN).		(T. S. HUNT).
CaO, SO^3.	0.26		undet.
CaCl.	10.47		9.205
MgCl.	10.50		9.484
KCl.	3.38		
KBr.	0.45	NaBr.	undet.
NaCl.	8.75		17.400
Water.	66.19		
	100.00		

* Salt spring from near Bay of Quinte.

GOESSMANN argues that this resemblance shows the probability that such mineral waters "originate from mother-liquors or bitterns of the saline residue of marine evaporation of the Silurian age."

In the same paper, GOESSMANN, after referring to the views of KARSTEN, upon saline springs, as published by him in 1847, that these springs originate usually from rock-salt of Primitive (volcanic) origin, and are modified in composition by subsequent chemical action upon strata through which they effect their exudation; and that therefore the presence of chloride of calcium, with other compounds, was but accidental, says: that the presence of this chloride has since been recognized "as especially characteristic of the salt deposits of ante-tertiary dates. Consequently these have been considered as a product of the constant admixture of the oceanic waters of preceding geological periods; while on the other hand its absence in our present Ocean and in most salt deposits of a more recent date, is an established fact." He doubts not that the composition of the Ocean has changed and is now changing, yet he asserts "that our ideas concerning the main features of the Primitive or Silurian Oceans are still vague, and especially so upon this point" (*i. e.* the presence or absence of chloride of calcium).

In connection with these questions, it has occurred to me that EBELMEN's results (*Annales des Mines* [4], xii, 67) may have importance. He there shows that in the alteration and subaerial erosion of trap, five-sixths of the lime, while not more than one-third of the magnesia and but half the alkalies, are lost. It is to be remembered that in our present Ocean the total lime is but about one-third of the total magnesia.

Deferring for the present some views of my own upon these points, I shall close this chapter with a brief remark upon another important element entering into these great and complex problems.

To a superficial observer it might appear that, as the Ocean continually receives mineral matter in solution, and continually evaporates, it should become more and more charged with each of its dissolved constituents. This view, however, requires great modification, and here we find one of the grandest illustrations of the governing influence of Life, according to my proposition at the outset of this paper. For example, as stated above, the continuous predominance of magnesia over lime in our present Ocean, notwithstanding that rivers constantly pour in several times more of the latter than of the former. Through Zoic influence numerous other important constituents of the oceanic waters are eliminated and converted into permanently solid and insoluble forms, and thus constantly removed, in many cases quite as fast as they are supplied. Among these may be mentioned *carbonic acid*, *sulphur*, *phosphorus*, *potash*, *iron* and *silica*.

II. Zoic History; from a Chemical View-Point.

This chapter of the memoir relates to the demonstration, illustration, and general elucidation of my proposition of the ruling influence of Life in the Chemical History of Geological development. The subject is so vast that I have judged it proper not to make a vain attempt to condense it into a compass appropriate to the present occasion. Dana purposely omits this and its related subjects from his great work, on account of the "large amount of space" that would be required (Dana's Geology, p. 604).

I will but say that the general scope of my attempt is to show that a consistent theoretical scheme of Zoic development, including the *breaks* in Zoic history, and other readings of the geologic revelation, may be based on the study of the progressive (direct and indirect) influences of Life itself on the elements of the Earth, and on the forces that move and transform them. I claim that we have here an almost new and unexplored field of investigation, the working of which is to yield us results quite as positive, valuable, and wonderful as those of the study of fossils, and that the two studies will be found supplementary and altogether essential to each other. I start of course with my primary postulate, offered in the preceding chapter, that Life found the Earth in a condition of completed combustion, and that it has been the sole Oxygen-Maker, and Deoxidizer. Herein Life has done *no work*, it has but directed or governed the working forces of the Solar Emanation, in reversing the opposing influences of chemical affinity. It is a master, not a laborer. In my philosophy I regard neither vitality nor affinity as forces capable of transforming matter. They are both rulers of the forces that transform matter. Their reigns are antagonistic and alternating with each other. This is a subject which I propose to treat elsewhere.

In gradually impregnating the atmosphere with active oxygen, Life has profoundly altered, in an infinity of ways, the whole chemical status of the earth's crust, and, moreover, by substitution of oxygen for the preëxisting carbonic acid, the weight and density (though not the volume) and a multitude of other physical conditions of the atmosphere, have been changed.

As examples, I shall select, almost at random, one or two of the striking geological views which grow out of such chemical considerations. As to the mode of formation of *petroleum*, many speculations have been offered; but the following *fact* is here pointed out for the first time. The petroleum-bearing strata antecede the larger mass of the Carboniferous. It is clear, therefore, that the oxygen corresponding to the carbon, and part of the hydrogen, of the huge masses of coal of subsequent Carboniferous strata, as well as of the fossil matters since deposited (the Tertiary lignites for example) and of all now existing vegetation, had not then, as yet passed into the atmosphere. In an atmosphere so poor (comparatively) in oxygen, could decay and eremacausis proceed with the rapidity and intensity of our day? Would they not rather be necessarily so slow and imperfect that liquid products would be formed instead of gaseous? Of course the greater content of carbonic acid in the atmosphere during the days of Petroleum-Genesis (represented now by not only all the fossil matter, but by all the limestones, dolomites and chalybites since formed) may have contributed its modifying influence.

As another example of the intermitting sway of Life over the successive chemical conditions of the telluric surface, I introduce here what appears to me to be a new and prolific generalization relating to the oscillations of oxygen between carbon and iron in past ages. In addition to the former postulates of my Zoic Theory of Chemical Geogony, I may in fact here enounce another. In a previous paragragh, I have presented the induction: *In the Prozoic Era, all the Potential Oxygen of the Earth's surface was in combination, either with Iron in the Hydrosphere, or with Carbon in the Aerosphere.*

The new postulate I have to present is as follows:

A large part of the Potential Oxygen has been swinging like a pendulum, under the alternating impulses af Vitality and Affinity, between Iron and Carbon, since the Zoic Dawn.

The discussion of this Postulate would lead me too far on this occasion. To illustrate it I shall but present the following scheme, constructed in tabular form, to represent the influence of the principle upon the chemical state of the Iron-Oxides in the oceanic sediments of the successive geological days.

TABLE OF SECULAR GEOLOGICAL OXYGEN-OSCILLATIONS.

AGES.	DAYS.	GEN'L CONDITION OF IRON.
PROZOIC, ...	. . . .	*Ferric.*
EOZOIC, ...	. . . .	*Ferrous.*
LOWER SILURIAN, .	Potsdam, ...	*Ferric.*
	Calciferous, . . .	*Ferrous.*
	Trenton, ...	
	Hudson River, . .	
UPPER SILURIAN, .	Oneida, ...	*Ferric* (though marine); [Red Shales and Sandstones and Red Iron Ores].
	Medina, ...	
	Clinton, ...	
	Niagara, ...	
	Salina, ...	*Ferrous.*
	Lower Helderberg,	
DEVONIAN, ...	Oriskany, ...	*Ferric* (?)
	Upper Helderberg,	*Ferrous.*
	Hamilton, ...	
	Chemung, ...	
	Catskill, ...	*Ferric* (for 6000 feet).
CARBONIFEROUS, . .	. . . .	*Ferrous.*
PERMIAN, ...	. . . .	*Ferric.*
	. . . .	
MESOZOIC, ...	Triassic, ...	
	Jurassic, ...	*Ferrous.*
	Cretaceous, . . .	
CENOZOIC, ...	Tertiary, ...	*Ferric.*
	Anthropozoic, . .	

III. CHEMICAL REVELATION OF A FINAL ZOIC CATASTROPHE.

In the course of the above attempts to fathom the Chemistry of the Past, indications have become apparent to me, which are applicable to the Future: and which seem to be of truly vital interest to the human race. There are chemical changes now active on the Earth's surface, easily demonstrated, whose continuance must inevitably bring about the final extinction of man, and ultimately of all other life upon our planet.

A single one, among the most important and inevitable of these, I propose here to explain.

What furnishes the actual fundamental chemical nutriment or pabulum, of vital existence? No chemist will contradict, when I say that it is the carbonic acid of the atmosphere. How long is this going to last? This question many will regard as absurd, having been taught that it is restored at least as fast as (possibly at the present day faster than) it is consumed. The notion, stereotyped in the text-books, is that, whatever may have been in the past, an equilibrium has been reached during the age of man, between production and consumption. This cannot be, unless there can be shown a mode of restoration corresponding, and equivalent to, each mode of consumption. There is one such mode, however, still active and continuous, and without sign of cessation, which will ultimately exhaust the atmosphere of its carbonic acid, and thus put an end to organic life. This agency is itself due and has ever been due to vital influences. It seems part of the great law of Zoic development that Life slowly evolves the causes of its own ultimate extinction on the Earth. The agency I refer to is that by which marine animals with calcareous shells or skeletons secrete carbonates from the ocean water, the carbonic acid of these carbonates having been originally derived from the atmosphere. Such carbonic acid thus passes into solid forms, permanent and for ever unavailable thereafter. This is where the great machine runs down, and Affinity obtains its final victory over its mysterious antagonist Vitality.

Whenever the last molecule of carbonic acid produced from the combustion of all the carbon on the Earth shall have been locked up in this shape, no form of life now known to us can any longer be possible, and the present Zoic Cycle must end. Comparatively and geologically speaking, the end is near; though millions of years may yet intervene. But long before this end of *all* life, the atmosphere must gradually diminish in its capacity to produce food suitable for man. No human power that we can discern can avert this result. Man, by burning up the carbon stored in eras past in the Earth's *viscera*, is doing his utmost to preserve the status of the machine, possibly even partially and temporarily *rewinding* it (here is a curious topic for speculation); but it must still continuously run down. In the oceanic depths, this precious constituent of the air, in which we literally, in a higher than poetic sense, "live, and move and have our being," is continually undergoing

"A sea-change
Into something rich and strange,"

never to reappear in form available to life, until indeed that time shall arrive, when "the elements shall melt with fervent heat;" and when, under the influence of this heat, the calcic and magnesic carbonates shall be converted into igneous silicates, rendering up again the treasures of carbonic acid in their marble grasp; the atmospheric oxygen—representative of Affinity, enemy of Vitality—shall also then be at least partially withdrawn by oxidation of sulphides and of ferrous oxide; and the Earth be thus far advanced in preparation for a new Zoic Cycle.

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8. Notice of some New Tertiary and Cretaceous Fishes. By O. C. Marsh, of New Haven, Conn.

(Abstract.)

The fossils exhibited by Professor Marsh, and briefly described in this communication, consisted of the remains of several new species of fossil fishes from the Cretaceous and Tertiary formations of the United States, and nearly all were from the greensand of New Jersey.

Among the remains of Tertiary fishes were specimens indicating two very diminutive species of Sword-fish, each of which was represented by the beak, or united premaxillaries. One of these fossils, for which the specific name *Histiophorus parvulus* was proposed, was a nearly perfect "sword," only about three inches long; which would indicate that the entire fish was probably not more than twenty inches in length. The beak in this species is slender and very pointed. It is compressed transversely, but has the lower surface nearly flat. The brush-like teeth on this portion are reduced to two narrow bands. The remaining surface of the sword is irregularly striated. This interesting specimen was found in the Eocene greensand of Monmouth County, New Jersey, at the pits of the Squankum Marl Company, and was presented to the Museum of Yale College, by O. B. Kinne, Esq.

The second small species of Sword-fish apparently belongs to a new type, allied to the extinct *Cœlorhynchus* of the Eocene. The beak resembles in general form that of *Cœlorhynchus*, but is much smaller, tapers more rapidly, and has the inferior surface flattened, and marked by two shallow grooves. Like the rostrum of that genus it has a double cavity at the base, and a single one through the main portion of the shaft. The upper surface is also fluted, but much more delicately than in any known species of *Cœlorhynchus*. When entire the beak was apparently not more than two and a half inches in length, and the whole fish probably did not exceed fifteen inches, which is by far the smallest sword-fish known. For the extinct genus represented by this specimen, the name *Embalorhynchus* was proposed, and the species was called *Embalorhynchus Kinnei*, after the discoverer, O. B. Kinne, Esq., whose explorations in the Tertiary of New Jersey have brought to light many interesting fossils. This specimen, also, was found in the Eocene greensand, at the pits of Squankum Marl Company.

A new species of *Phyllodus*, the first discovered in this country, was likewise announced, and briefly described under the name *Phyllodus elegans*. It was represented by a pharyngeal, dental plate, with the teeth in an excellent state of preservation. This specimen differs from the corresponding part in the known species of this genus, in its form, which is subtriangular, and especially in its much smaller size, as it is but nine and a half lines in length, or scarcely more than one-fourth as large as the smallest already described. In the number and position of the various teeth it appears to most nearly resemble *Phyllodus toliapicus* Ag. (Poissons fossiles, Vol. II, pl. 69 *a*, fig. 1) from the London clay, but differs from that species in having the central teeth proportionally much more elongated and the lateral ones less numerous. This unique specimen was found in the Eocene greensand at Farmingdale, New Jersey, in the pits of the Squankum and Freehold Marl Company, and was presented to the Yale Museum, by Major A. J. Smith, the Superintendent.

A second and larger species, apparently of the same genus, was represented by the central portion of the corresponding dental plate. It is readily distinguished from all the described species of *Phyllodus*, by the unusual thickness of the teeth, and by the fact that the longest of the series are considerably curved, so that the crushing surface of the plate is concave transversely. This species was named *Phyllodus curvidens*. The specimen on which it is established was found near Shiloh, Cumberland County, New Jersey, in the Miocene Marl, and is, therefore, the most recent known representative of this type of fishes.

Another new species from the Tertiary was indicated by the palatal plate of a fossil ray, for which the name *Myliobates bisulcus* was proposed. It differs from the species of this genus already described, in having the central row of teeth marked along the median line by a deep groove. In other respects the dental surface is remarkably smooth and flat. This specimen is from the Eocene greensand of Monmouth County, New Jersey, and also belongs to the Museum of Yale College.

Among the Cretaceous specimens exhibited were several ichthyodorulites, which evidently belonged to Chimæroid fishes, and indicated a species new to science. One of these was a dorsal spine, nearly perfect, and about fourteen inches in length. It is somewhat curved, and remarkably slender, being but nine lines in antero-posterior diameter at the base, and tapering regularly to the apex. It is compressed transversely, suboval in general outline, and has the posterior surface slightly concave in the lower portion. The upper half of this surface is armed with two rows of very sharp, decurved teeth, while the corresponding part of the anterior face has a sharp cutting edge, which toward the distal end is finely serrated. The sides of the spine are smooth, or faintly striated. This specimen was found by John G. Meirs, Esq., in the upper Cretaceous marl bed, near Hornerstown, New Jersey, and the species is named *Dipristis Meirsii*, in honor of the discoverer. Fragments of this species, of much larger size than the specimen described, are not uncommon in the same geological horizon in other parts of the State.

Another Cretaceous species, *Enchodus semistriatus*, was described from a number of shed teeth. The most perfect of these was fourteen lines in length and three and a half lines in diameter at the base. It was slightly sigmoid in shape, compressed, and has in front a sharp cutting edge which is minutely denticulated. The rounded posterior surface was marked by delicate striæ, except near the apex, which is furnished with a distinct barb. Some of the smaller teeth were more nearly straight, and apparently without the barb. All the specimens of this species yet discovered are from the lower Cretaceous Marl bed of New Jersey.

II. ZOOLOGY.

1. Observations on Phyllopod Crustacea of the Family Branchipidæ, with descriptions of some New Genera and Species, from America.* By A. E. Verrill, of New Haven, Conn.

The Phyllopod Crustacea are among the most interesting of the Entomostraca, as they are also by far the most beautiful. In size the species generally exceed those of most other groups, except the Limuloids and Cirripeds. The numerous peculiar natatory appendages, which are moved with a peculiarly graceful undulatory motion, give them an elegant appearance when in motion. In this country they have hitherto been but little

* An abstract of this communication was printed in the "American Journal of Science," xlviii, p. 244, Sept., 1869, and reprinted in "The Annals and Magazine of Natural History." In the present paper many alterations and additions have been made.

studied, and doubtless many more forms remain to be discovered.

Among those already described from North America, are two species of *Apus*, *A. glacialis*, from the Arctic regions, and *A. longicaudatus* Leconte, from the Rocky Mountains.

In the following remarks those belonging to the Branchipidæ are alone considered.

ARTEMIA Leach, 1819.

This genus is characterized by having eleven pair of four-jointed branchial "feet" or fins along the sides of the body, the middle ones being longest; each joint bearing flat branchial appendages, ciliated by sharp setæ, as in the other genera of the family. The abdomen is slender, six-jointed, the last joint long, terminated by two small projecting appendages, each bearing six to ten plumose setæ. The first abdominal segment bears the external sexual organs of the male, and a short, dilated, ovigerous pouch in the female. In the male the head bears in front a pair of large, three-jointed hooks or clasping organs, each of which has on the inner side of its basal joint a small, rounded appendage; a pair of slender antennæ, just back of these, terminated by two or three minute setæ; a pair of pedunculated compound eyes, and a dark spot on the middle of the head, which is the remains of the single eye of the young. The mouth below is provided with a broad labrum, a pair of mandibles, two pairs of jaws, and a pair of lateral papillæ. In the female the head lacks the stout claspers, which are replaced by a pair of comparatively small, simple, horn-shaped organs.

According to Dr. Baird,* the genus *Eulimene* Latreille, 1817, was based on specimens of *A. salina*, which were badly preserved and erroneously described. That name was, however, preoccupied among Acalephs.

This interesting genus is remarkable for its habit of living and flourishing best in very saline and alkaline waters, such as the natural salt lakes of Egypt, Utah, etc., and the artificial brines formed by the evaporation of sea-water by exposure

* Monograph of the Family Branchipodidæ, etc., in Annals and Mag. Nat. History, vol. 14, p. 216, 1854.

to the heat of the sun, as in England, France and the West Indies.

The species first made known, *A. salina* Leach (*Cancer salinus* Linn.), was first described by Schlosser,* who found it in great profusion in the brines of Lymington, England. Linné indicates it also from the salt lakes of Siberia (perhaps a distinct species,† and probably the same as that observed by Pallas‡ in great numbers in the Great Schimélée). More recently it has been described from the salterns of southern France, at Montpellier, etc.§ The genus has been found also in the Lakes Goumphidich, Amaruh and Bédah in Egypt, which are reported to be both very saline and alkaline, their bottoms being "covered with a layer of crystals of carbonate of soda, sulphate of soda, and common salt," while the density of the water is stated at 1.255. The Egyptian species appears not to have been described as yet.‖ In the Antilles *A. Guildingi* Thompson occurs.¶ *A. Milhausenii* Edw. (Fischer sp.) is found in Lake Loak in the Crimea.** A few years ago Prof. B. Silliman presented to the Museum of Yale College a number of specimens of a new species, *A. Monica* V., which he collected in Mono Lake, California, where it occurs in great abundance

* Observations périodiques sur la physique, l'histoire naturelle et les beaux-arts, par Gautier, 1756 (with figures). An extract from this is republished in Annals des Sciences nat., 2e ser., t. 13, p. 226, 1840, in an elaborate description of the anatomy, development, habits, etc., of *Artemia salina* by M. Joly, illustrated by two excellent plates of the female and young. M. Joly failed to observe the male among more than a thousand females, and therefore doubted whether the sexes were distinct, suggesting that the males, very well described by Schlosser, were only the young, although that author described them as clasping the females in the well known manner, but he did not observe the actual copulation.

See also an article by Thomas Rackett, in Trans. Linn. Soc. of London, vol. xi, p. 205, pl. 14, 1812 (figures very bad); Thompson, Zoological Researches, No. 5, p. 105, t. 1 and 2; W. Baird, Nat. Hist. of the British Entomostraca, p. 61, tab. ii, figs. 2-4 (figures very good, but the specimens probably not full-grown).

† *Polyartemia forcipata* Fischer, is from the rivers of Siberia and also from Lapland. It resembles *Artemia*, but has nineteen pairs of natatory appendages and also peculiar appendages to the male claspers, with the second joint divided.

‡ Voyage en différentes provinces de l'empire de Russie, t. ii, p. 505 (t. Joly).

§ M. Payen, Note sur des animaux qui colorent en rouge les marais salans, Ann. des Sci. nat., 2e ser., t. 6, 1836, p. 219 (contains experiments on the effects caused by altering composition and density of the water); also op. cit., t. 10, 1838, p. 315; Joly, op. cit., t. 13, p. 225, 1840 (see above); Milne Edwards, Crustacés, t. iii, p. 369, 1840.

‖ Audouin, Ann. des Sci. nat., 2e ser., t. 6, 1836, p. 230.

¶ Thompson, Zool. Researches, fas. 7, pl. 1, figs. 11-12.

** Edwards, Crustacés, t. iii, p. 370, 1840.

associated with the larvæ of *Ephydra*.* The water of this lake is very dense, and not only very saline, but also so alkaline that it is said to be used for removing grease from clothing. I have been unable, however, to find any reliable analysis of this water. It is said to contain, also, biborate of soda. Prof. Silliman informs me that the genus also occurs in Little Salt Lake. It occurs in great abundance in Great Salt Lake, Utah, as I am informed by Prof. D. C. Eaton, who obtained specimens there during the present summer.† The water of Great Salt Lake has usually been described by travellers as destitute of all life, but according to Prof. Eaton it contains not only an abundance of *Artemiæ*, but also various other small animals, insect larvæ, etc. The density of the water is stated at 1.170°, but doubtless varies according to the season.‡ It yields, according to Dr. Gale, over 22 per cent of solid matter,§ while the Syracuse Saline, one of the richest natural brines in the United States, contains but 19.16 per cent.‖ A few weeks ago Mr. Oscar Harger discovered another new species, *A. gracilis* V., near New Haven, under very peculiar circumstances. On the long wooden bridge across West River and the extensive salt marsh on the West Haven side, are placed large wooden tubs filled with water from various pools on the marsh, to be used in case of fire. By long exposure to the sun and air the water in these becomes concentrated and thus furnishes suitable stations for the rapid increase of *Artemiæ*. On examining the tubs the first of August I found eight of them partly filled with water, in six of which the

* Verrill, Proceedings Boston Soc. Nat. Hist., vol. xi, p. 3, 1866 (the larvæ were wrongly referred to *Eristalis*); Packard, on Insects inhabiting salt-water, Proc. Essex Inst., vol. vi, p. 41, 1869.

† This species has since been examined and found to be a distinct species, *A. fertilis*.

‡ The density of the water of the Atlantic Ocean is stated at 1.026; that of the Dead Sea 1.130 to 1.227.

§ This solid matter, according to Dr. Gale (Amer. Journal Science, II, vol. xvii, p. 129), has the following composition:

Chlorid of sodium,	20.196
Sulphate of soda,	1.834
Chlorid of magnesium,	.252
Chlorid of calcium,	trace
	22.282

‖ For analyses of several of these brines, see Dana's System of Mineralogy, 5th edition, 1868, p. 118.

Artemiæ were found in abundance, though more numerous in one than in any of the others. In one tub, in which the water had a decidedly milky appearance, they were so abundant that hundreds could be obtained in a few minutes. The water in some of the other tubs containing them was of a reddish or brownish hue, or about the color of weak tea. In two no Artemiæ could be seen, and in these the water appeared to have been more recently renewed. Search was made in the pools from which the water had been taken, but no *Artemiæ* were found, though doubtless from these places the progenitors of those inhabiting the tubs must have been taken. It is probable that in the pools they exist in very small numbers, being kept in check partly by various small fishes and other enemies, and partly by the unfavorable character of the water, while in the tubs the density of the water is more favorable for their rapid increase, and unfavorable or fatal to their enemies.* The water from the tubs, when examined with a high power of the microscope, was found to be filled with immense numbers of infusoria of various kinds, such as monads, vibrios and bacteria, most of which were so small as to be distinguishable only as moving points with a $\frac{1}{8}$ inch objective.

In the salterns of France the *Artemiæ* are associated with immense numbers of a monad, usually bright red in color, which has been named *Monas Dunalii* by Joly, who attributes to it the red color which the brine assumes just before crystallization,† as well as the red color observed in the *Artemiæ*, which doubtless feed upon it, as well as upon various other living infusoria, and dead animal and vegetable matter of various kinds.‡ The *Monas Dunalii* appears in abundance in the water having the density most favorable for *Artemia*, but increases in far greater proportion in the still denser, nearly or quite saturated brine in which *Artemia* does not live. The observations of Payen and Joly show that the *A. salina* of

* The density of the water in two of the tubs containing most *Artemiæ*, was 1.065, equivalent to a brine containing 9.07 per cent of salt. One of those tested was brownish, the other milky.

† Recherches sur la Coloration en rouge des Marais Salans Méditerranéens, par M. Joly, Annals des Sciences naturelles, 2e ser., t. xiii, 1840, p. 266.

‡ According to M. Joly, op. cit., p. 262, a beetle, *Hydroporus salinus* Joly, also inhabits the salterns, where the water has a density of 6° or 7° Baumé, and preys upon the *Artemiæ*.

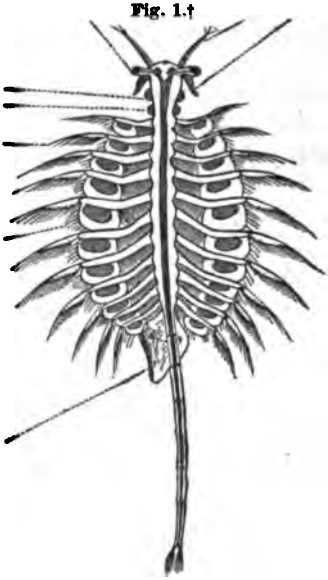
France can exist in waters varying in density from 4° to 20° Baumé, but that they flourish best in those that have a density of 10° to 15°.* According to Rackett those of Lymington do not live in the water which is undergoing the first stage of concentration, but only in the pans of concentrated brine, containing about "a quarter of a pound of salt to the pint."

Our *A. gracilis* can exist without apparent inconvenience when the water in which they occur is diluted with an equal bulk of fresh water, as well as when it is much concentrated by evaporation. The water in which they were found varies in density from 1.060 to 1.065.

ARTEMIA GRACILIS Verrill.

American Journal of Science, xlviii, p. 248, September, 1869; reprinted in Ann. and Mag. of Nat. History, Vol. iv, p. 331, 1869.

Fig. 1.†



Body slender, in the male about .3 of an inch long; in the female .4. Claspers of the male relatively long and powerful, first joint thickened, with a distinct angle at the articulation on the outside and a short, rounded, nearly semicircular process on the inside near the base, about its own diameter from the base; second joint broad, flattened, continuous with the third joint, strongly curved, outline nearly regularly convex on the outside, until near the middle it suddenly bends inward forming an obtuse angle, beyond which the outline is concave to the last articulation, where it becomes again convex, forming on the last joint a slight, rounded angle,

* 4° to 20° Baumé is equivalent to a density of about 1.03 to 1.16; 10° to 15° = 1.075 to 1.117. A brine having a density of 1.020, which is nearly that of sea-water, contains about 2.766 per cent of salt; one of 1.160 contains 21.219 per cent; one of 1.075 about 10.279 per cent; 1.117 about 15.794 per cent.

† Figure 1.—*Artemia gracilis*, female, enlarged; drawn from living specimen by J. H. Emerton.

the inner edge is nearly straight, or but slightly concave, to the last articulation, where there is a slight but distinct angle; last joint triangular, longer than broad, tapering to the acute, slightly excurved point. Antennæ slender, elongated, reaching beyond the first articulation of the claspers, terminal setæ minute. Abdomen slender, smooth, the terminal lobes small, longer than broad, broadly rounded at the end, slightly constricted at the base inside, each bearing usually 7 or 9 plumose setæ, the central ones much the longest. Ovigerous pouch of the female, when seen from below, flask-shaped, the neck extending backward and downward, short, thick, subcylindrical toward the end, the body of the "flask" short, thick, swollen laterally, broader than long, the sides terminating outwardly in a small, triangular, sharp tooth, sometimes showing a minute spine. This pouch is generally filled with numerous large, brownish eggs.

Fig. 2.*

Color generally reddish, flesh-color, or light greenish, translucent; the males usually lighter, greenish white, the intestines generally showing through as a dark reddish or greenish median line; eyes very dark brown, or black; ovaries often whitish, along each side of the abdomen.

An adult male gives the following measurements:—distance between eyes 1.81^{mm}; breadth of head .76; length of eye-stalks .62; length of first joint of the claspers .91; its breadth .72; breadth of its appendage .18; length of second and third joints from outer edge of first articulation to the tip 2.48; greatest breadth .87; breadth at last articulation .72; length of last joint 1.05; length of last joint of abdomen, exclusive of appendages, 1.00; its breadth .31; length of prece-

*Figure 2.—*a*, head of *A. gracilis*, male, viewed from below, showing part of the mouth organs, the eyes and the claspers; *b*, abdomen of the same, with male organs; *c*, head of *A. Monica*, showing eyes, antennæ and claspers; *d*, caudal appendages of the same. All these figures are from camera-lucida drawings by the author, and enlarged seven diameters.

ding joint .42; its breadth .37; length of terminal appendages .21; breadth .096; length of longest setæ .70.

New Haven, Conn. Charlestown, Mass., on railroad bridge across Charles River in tubs of concentrated sea-water.*

ARTEMIA MONICA Verrill, op. cit., p. 249.
Figure 2, *c. d.*

Form similar to that of the preceding species, but a little larger and stouter. The largest female is 13mm (.52 of an inch) long, the abdomen being 6mm; and 5mm across the branchial feet in their natural, partly extended, position. The largest male is 11.5mm (.45 of an inch) long, the abdomen being 6mm. The claspers of the male are relatively stouter, the hook or outer two joints being much broader, more triangular, and less elongated. The inner edge of the first joint, as seen from below, is regularly convex, bearing the appendage on its most convex part, and not so near the base as in *A. gracilis*, the distance being about twice the breadth of the organ, which is about as broad as long and regularly rounded. At the articulation the outer edge of the joint projects as a distinct angle. The second and third joint together have a nearly triangular form, the breadth being about half the length; the outer edge is regularly rounded, shorter than in the preceding; it forms little more than a right angle with the front edge, which is nearly straight or a little concave, sometimes slightly convex at the last articulation, but not forming a distinct angle there; the inner edge of the hook is a little concave on the first joint, becoming convex at the last articulation, where there is a distinct but very obtuse angle. The last joint is almost regularly triangular, about as broad as long, tapering to an obtuse point, the inner edge being a little convex. The antennæ are very slender and do not reach the first articulation of the claspers. The caudal appendages are smaller than in *A. gracilis*, and scarcely longer than broad, rounded at the end, terminated by nine or ten very slender pulmose setæ. The egg-pouch of the female is broad flask-shaped, strongly convex in the middle below, the sides not forming such sharp angles as in *A. gracilis*.

* During the progress of the meeting Dr. G. H. Perkins discovered this species in great abundance in tubs of water taken from the Charles River and kept on the Eastern Railroad bridge. Professor Agassiz also stated that he had formerly observed this or a similar species in the salt vats at Cape Cod.

The English specimens of *A. salina*, as figured by Baird, differ from both the preceding species in having longer, more curved, and sharper clasping hooks, and the basal appendage more elongated; the egg-pouch, though badly figured, is of a very different form. The French specimens, as figured by Joly, appear like a distinct species, the egg-pouch being of a very different form, and the caudal appendages very much longer and larger than in either of our species, while Baird's figure represents them as very small; but his specimens appear to have been smaller, and may have been immature, for these species begin to breed before they are half-grown. Whether the French species be distinct from the English can only be determined by additional examinations, especially of the male, for the male of the former appears not to have been figured hitherto.

ARTEMIA FERTILIS Verrill.

American Journal of Science, Vol. xlviii, p. 430, Nov., 1869.

This species grows to a larger size than I have observed in either of the others, some of the specimens being full .75 of an inch long. The claspers of the male (fig. 3, *a*) are stout, with the second joint broader and more triangular than in either of the preceding species. The outer angle of the second joint of the claspers is very prominent, and the outline from thence to the tip is decidedly concave, in this respect resembling those of *A. gracilis* more than those of *A. Monica*. The caudal appendages (fig. 3, *b*) appear to be shorter than in either of the others, but this character varies considerably with age in all the species of this genus.† Great Salt Lake, Utah,—Sereno Watson; D. C. Eaton; S. A. Briggs.

Fig. 3.*

*Figure 3.—*a*, head of *Artemia fertilis* V., male; *b*, caudal appendages. Camera-lucida drawings by the author from alcoholic specimens, enlarged seven diameters.

† For this reason several nominal European species, established mainly on differences in the caudal lobes and setæ, are probably only the young of others, or all perhaps of *A. salina*, especially since those with small caudal lobes and few or no setæ, are described as small; as for example, *A. Milhausenii*, *A. arietina* and *A. Koppeniana* (Fischer species).

Another species has recently been made known from New South Wales, *A. proxima* King, Entom. Soc., N. S. Wales, Vol. i, pl. xi.

This species occurs in vast numbers in the very dense waters of Great Salt Lake, together with the larvæ of *Ephydra*.

BRANCHIPUS Shœffer.

Branchipus Shœffer, Elementa entomologica, Pl. 29, figs. 6, 7, 1766, (type, *B. pisciformis* = *B. stagnalis* Linn. sp.).

Branchipus (*pars*) Lamarck, Latreille, Leach, Edwards.

Chirocephalus (*pars*) Dana (*non* Bénédict Prévost, 1803; Jurine, Thompson, Baird).

Under the name, *Branchipus*, at least five or six generic groups have been confounded by various authors.

Branchipus should be restricted to the original species described by Shœffer and the allied species, of which *B. stagnalis* (Linn. sp.) is one, if not identical with *B. pisciformis*, as is generally supposed.

As thus restricted the genus is characterized by the stout two-jointed claspers of the male, with or without a tooth near the base of the hook, the basal joint being swollen; by having a pair of simple appendages resembling antennæ between the bases of the claspers in front (fig. 4, *c*); by the large, thick, oval egg-pouches of the female, and, apparently, by the structure of the branchial organs. The typical species have, in addition to the pair of slender antenniform organs, a short bilobed organ (*d*) between the bases of the claspers in front.

Fig. 4.*

The type of this genus is *B. stagnalis* of Europe (fig. 4, head of male). It is doubtful whether any other described species can be properly referred to the genus.

STREPTOCEPHALUS Baird.

Monograph of the Family Branchipodidæ, etc., in Annals and Mag. Nat. History, vol 14, p. 219, 1854.

Heterobranchipus Verrill, American Jour. Science, xlviii, p. 250, 1869.

Body and caudal appendages as in *Branchipus*; natatory or-

* Figure 4. — *Branchipus stagnalis* Latreille, head of male, enlarged; *a*, antennæ; *b*, eyes; *c*, antenniform organs; *d*, bilobed organ at base of claspers; *e*, claspers. From Latreille after Shœffer's figure of *B. pisciformis*.

gans in eleven pairs. The claspers of the male are long, three-jointed, tortuous or twisted; the terminal joint subdivided more or less, into two or more branches, or bearing slender appendages. Near base of claspers are two antenniform organs. Front of head with a bilobed organ, between bases of claspers. Male organs long, slender, complex. Egg-pouch elongated or conical.

This genus includes *S. cafer* (Loven sp.) Baird, from the marshes of South Africa; *S. similis* Baird, from St. Domingo, West Indies; *S. torvicornis* (Waga sp.) Baird, from near Warsaw, in the latter the claspers are said to be as long as all the rest of the body; *S. rubricaudatus* (Klunzinger sp.) from Kosseir, near the Red Sea.* The last species has very long slender claspers in the male; the first joint bearing a slender antenniform organ at its outer end; the second with three slender teeth-like processes on the outer side; the third is crooked, subdivided at the end into two long crooked branches, of which the inner is much the longest, sickle-shaped, and serrated on the inner edge. The external male organs are comparatively small and simple. The egg-pouch is long, slender, and beaked at the end. This and *S. torvicornis* are closely allied and should be considered typical species of *Streptocephalus*, while *S. cafer* and *S. similis* might well be separated, as a subgenus, at least, under the name of *Heterobranchipus*.

CHIROCEPHALUS Prevost, 1803.

This genus, established for *C. diaphanus*, is evidently very distinct from both the preceding. The typical species is large, stout, and remarkable for the singular appendages between the claspers of the male, on the front of the head. These consist of two long, ligulate, fleshy processes, serrated on each side, which coil in a spiral beneath the head, but when extended, as in copulation, reaching beyond the claspers; attached to the outer side of each of these are four long processes, strongly serrate on the inner edge, and near the base another large, broad, thin, subtriangular appendage, its edges strongly serrate, especially in front, capable of folding up like a fan when not in use. The claspers have a much swollen basal joint, a

* Zeitschrift für Wissensch. Zoologie, xvii p. 23, Taf. iv. figs. 1–9, 1867.

strongly serrate tooth on the inside of the base of the second joint, which beyond this is slender and regularly curved. Egg-pouch long-oval, large and thick; caudal appendages large; male organs and branchiæ peculiar.

C. diaphanus Prev., inhabits fresh-water pools in France, Switzerland and England. It is well described and figured in Baird's British Entomostraca, p. 39, tab. III and IV.

Dr. Baird, in his monograph, refers to this genus the following species: —

C. claviger (Fischer, sp.), from Siberia; *C. birostratus* (Fischer. sp.), near Charkow, Russia; *C. lacunæ* (Guérin, sp.), near Fontainebleau; *C. Middendorffianus* (Fischer, sp.), from Siberia and Lapland.

The last appears, however, to belong rather to our *Branchinecta*, and *C. lacunæ* ought, perhaps, to form the type of a distinct genus, since it lacks the complicated appendages of *C. diaphanus*. This reference of *C. birostratus* is also scarcely satisfactory; it may be nearer to our *Eubranchipus*.

EUBRANCHIPUS, gen. nov.

Body robust, with eleven pair of natatory appendages. Male with large head and very stout claspers; first joint of clasper much swollen, capable of retracting the basal portion of the second joint into their cavity; second joint stout at base; in the typical species with a large tooth on the inside, the outer portion tapering, rather obtuse.

Front of head, between the bases of the claspers, bears two thin, flat, tapering appendages, serrated on the edges and transversely striated or jointed. Caudal appendages long lanceolate, with numerous plumose setæ. Egg-pouch short and thick, swollen and broad-oval.

Besides the following species this genus appears to include *Branchipus spinosus* Edw., from a salt lake near Odessa, but the latter appears to have no tooth at the base of the second joint of the claspers.

EUBRANCHIPUS VERNALIS Verrill.

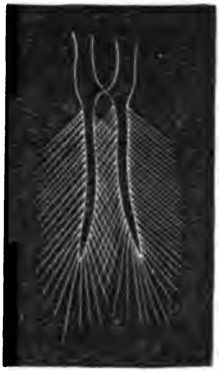
Branchipus vernalis Verrill, op. cit., p. 251.

Form rather stout, large; the full grown females are 23^{mm}

(.90 of an inch) long, the abdomen being 14^{mm}; and 6.5^{mm} wide across the branchial organs in their natural position; breadth of head across the eyes 4^{mm}. A large male is 22^{mm} (.86 of an inch) long, the body 12^{mm}; the breadth of head across eyes 5^{mm}; the entire length of claspers 8^{mm}. The claspers are very large and strong, the basal joint much swollen with a soft integument, capable of retracting the basal portion of the second joint into itself by involution of its outer edge; the second joint is elongated, broad and stout at base, with an angle on the outside, from which it rapidly narrows by strongly concave outlines on each edge, but most so on the outside; at the constricted portion, not far from the base, it bears a large, strong, very prominent, crooked, bluntly pointed tooth, which is directed inward and backward, not serrate on its outer side; beyond the tooth the rest of the joint is long and rather slender, curved outward and forward at base, having just beyond the tooth on the inside a distinct but very obtuse rounded angle, from which the outline slightly curves inward to near the tip, which is a little dilated and recurved. The basal portion, including the tooth, is retracted into the first joint in some

Fig. 5.*

Fig. 6.†



* Figure 5.—Head of *Eubranchipus vernalis*, male; *a*, serrated organs between the bases of the claspers; *b*, second joint of claspers; *c*, first joint, with the basal portion of the second joint somewhat retracted into its cavity.

† Figure 6.—Caudal appendages of the same specimen. Both figures are from camera-lucida figures by the author, enlarged six diameters.

specimens. On the front of the head between the basal joints of the claspers are two flat, lanceolate, short, ligulate, fleshy processes, with finely serrate edges, usually coiled down, but when extended scarcely more than half as long as the basal joint of the claspers. Antennæ small and very slender, tapering, reaching a little beyond the eyes. Caudal appendages long, rather narrow, slightly swollen at base, gradually tapering to the acute tips, and bearing along the sides, except at base, very numerous, long plumose setæ. Egg-pouches short, broad-oval, nearly as wide as long, slightly three-lobed posteriorly, the central lobe largest, sides extended and largely adherent to the sides of the abdomen, length 4^{mm}; breadth 3.5. Body flesh-color or pale red, the intestine darker red or greenish.

A large male gives the following measurements: length of first joint of claspers 4.62^{mm}; diameter 2.40; length of second joint 4.14; breadth at base 1.90; at tooth .72; in middle .52; length of tooth .90; its diameter .33; length of caudal appendages 4; breadth at base .33; in middle .20; length of setæ 2; length of antennæ 3.

New Haven, in stagnant pools,—J. D. Dana, D. C. Eaton, A. E. Verrill; Salem, Mass., April 19, 1859,—R. H. Wheatland, C. Cooke (from Essex Institute); Cambridge, Mass.,—A. E. Verrill.

This differs widely from all the described species of Europe, in the character of the claspers of the male and their appendages. *E. spinosa* resembles our species somewhat in the frontal appendages between the claspers, but lacks the conspicuous tooth at the base of their second joint. The shape of the egg-pouch in our species is also characteristic.

This is doubtless the species referred to by Dr. Gould under the name of *Branchipus stagnalis.** Dekay† copied the diagnosis of *B. stagnalis* (?) from a foreign work, and gave a figure of *Chirocephalus diaphanus*, copied apparently from Desmarets, pl. 56, which is itself a copy.

This species appears very early in spring, often in great numbers, in quiet pools. I have never seen it later than the middle

* Invertebrata of Massachusetts, p. 339.

† Natural History of New York, Zoology, Part I, Crustacea, p. 63, pl. ix, fig. 36.

of May, yet since the individuals seen in early spring are full-grown, it might, doubtless, be found also in autumn.

BRANCHINECTA Verrill, op. cit., p. 250.

Form rather slender, with the median appendages longest, so as to somewhat resemble *Artemia* in outline, but larger. Male with rather slender, pointed, rounded, two-jointed claspers; the basal joint somewhat enlarged, with an oblique row of small teeth on the inner side; the second joint curved, tapering, the inner edge usually finely serrulate. Front of head and base of claspers without other appendages of any kind. Caudal appendages slender, or narrow lanceolate, fringed with plumose setæ. Egg-pouch much elongated, and in some, if not all species, with lateral lobes at base.

BRANCHINECTA ARCTICA Verrill.

Branchipus (*Branchinecta*) *arcticus* Verrill, op. cit., p. 253, 1869.

Branchipus paludosus Packard, Invertebrate Fauna of Labrabor, in Mem. Boston Soc. Natural History, i, p. 295 (*non* Müller).

Fig. 7.*

Form slender; body short; abdomen elongated. A full sized male is 20^{mm} (.80 of an inch) long, exclusive of the claspers, the abdomen being 13^{mm}; the breadth between the eyes 3^{mm}. A female 20^{mm} long, with the abdomen 12^{mm}, has an egg-pouch 6.2 long. Branchial "feet" slender, elongated, the middle ones longest, 4 to 5^{mm} long when extended. Claspers of the male (fig. 7*a*) rather long and slender; the basal joint is but little swollen, elongated, regularly curved, with a small tooth or prominent angle at the articulation on the inside, and on the inner side a row of numerous small, distinct, sharp teeth, extending from the articulation about half way to the base, and arranged somewhat obliquely; second joint slender,

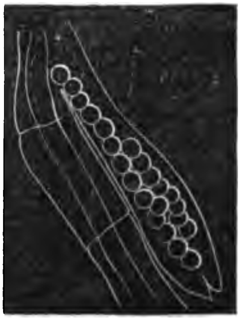
*Figure 7.—*a*, head of *B. arctica*, male, showing the jaws, eyes and claspers; *b*, same of *B. Grœnlandica*, except that the jaws are omitted.

regularly curved, tapering to a blunt point, the inner edge minutely serrulate. Front simply curved with no appendages. Antennæ slender, scarcely more than half the length of the basal joint of the claspers. Labrum long and narrow, mandibles stout, strongly curved, bluntly pointed. Caudal appendages (fig. 8) slender lanceolate, rather small, with long slender setæ. Egg-pouch (fig. 9) much elongated, slender, subcylindrical, beaked or slightly bilobed at the end, the upper or dorsal lobe longest; basal portion with two small rounded lateral lobes.

Fig. 8.*

A large male gives the following measurements: breadth between outer extremity of eyes 3.46^{mm}; diameter of eyes .66; length of basal joint of claspers 1.66; breadth .71; length of second joint 1.29; breadth at its base .46; width of mandibles at middle .66; length of caudal appendages .96; breadth at base .16; length of longest setæ .84 to 1^{mm}.

Fig. 9.†



Color of preserved specimens pale reddish, with dark green intestine. Labrador, at "Indian Tickle" on the north shore of Invuctoke Inlet, abundant in a pool of fresh-water.—Dr. A. S. Packard.

Dr. W. Baird in Ann. and Mag. Nat. Hist., vol. 14, 1854, p. 228, mentions imperfect specimens of a species brought from Cape Krusenstern by Sir John Richardson, which were probably of the present species. It was found with *Apus glacialis*.

BRANCHINECTA GRŒNLANDICA Verrill.

Branchipus (*Branchinecta*) *Grœnlandicus* Verrill, op. cit., p. 253.

A little stouter than the last; the largest male is 17^{mm} long exclusive of claspers, the abdomen being 10^{mm}, including caudal appendages. Claspers similar to those of *B. arctica* but

*Figure 8.—Caudal appendages of *B. arctica*, male.

†Figure 9.—Egg-pouch of *C. arctica*, containing eggs. All these figures are from camera-lucida drawings by the author, enlarged seven diameters.

more elongated, the basal joint less curved, and the second joint longer, less regularly curved, tapering more quickly at base and consequently more attenuated beyond the middle and with more slender tips, which are nearly straight. The tooth on the inside of the first joint is rather more prominent, but the row of teeth along the inside is similar. Caudal appendages stouter, tapering more rapidly. External male organs slender, curved outward, swollen at base. The largest female is not mature and the egg-pouch contains no eggs; it is small, slender, elongated, subcylindrical, beaked at the end.

Fig. 10.*

The largest male gives the following measurement: breadth between eyes 3.20^{mm}; length of basal joint of claspers 2.81; breadth .95; length of second joint 2.24; its breadth at base .76; length of caudal appendages .86; width at base .24; Length of setæ .76.

Greenland.—Dr. Chr. Lütken (from the University Zoölogical Museum, Copenhagen).

Of this species I have seen but four specimens, which were sent to Dr. A. S. Packard by Dr. Lütken, under the name of *B. paludosus* Müller. The latter appears to be quite distinct, judging from the figures; it is represented as having very slender, linear, caudal appendages. In the form of the egg-pouch, and the serration of the first joint of the claspers it is similar.

This species is very closely allied to *B. arctica*, and when a larger series of specimens can be examined it may prove to be only a local variety, but the specimens studied show differences that seems to warrant their separation.

Branchinecta paludosa (*Branchipus paludosus* Müll.) is also a northern species of this genus, allied to the two preceding, but differing from both, according to the figures and descriptions, as mentioned under the last. *B. ferox* (*Branchipus ferox* Edw., Crust., iii, p. 369) is from fresh-water near Odessa. The description is so brief and imperfect that its generic affini-

*Figure 10.—Caudal appendages of *Branchinecta Grœnlandica* V., male, enlarged seven diameters.

ties cannot be made out with perfect certainty, but it agrees better with this genus than with any other. *B. Middendorffiana* (Fischer sp.) from Siberia and Lapland, may also belong here.

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2. On the Trend of the Rocky Mountain Range North of Lat. 60° and its Influence on Faunal Distribution. By W. H. Dall of Washington, D. C.

About latitude 60° north, this great range nearly approaches the coast range, and about latitude 64° bends, in a confused mass of mountains, trending with the coast to the south and westward, and gradually coalescing and becoming merged with the Alaskan Range, which forms the backbone of the peninsula of Aliaska.

To the north of this there are no considerable elevations worthy of the name of mountains, except a few peaks of the Romanzoff Mountains. All the ranges of hills and mountains have the same general trend.

Bering Strait is only thirty fathoms deep, and although there is a deep ocean valley culminating in the mouth of Plover Bay to the westward of the strait, yet an elevation of one hundred and eighty feet would unite Asia and America with a dry plateau offering no obstacles to the migrations of animals and plants.

The old maps all represent the Rocky Mountains as extending to the shores of the Arctic Ocean, but this is incorrect.

The effect of this bend of that chain is to have a high broken plateau to the northward, by which the eastern birds, &c., pass to the shores of Bering Sea, while the characteristic west coast fauna is almost entirely excluded, though we have birds hitherto known from Europe, Siberia and Polynesia, breeding at the Yukon mouth with *Ampelis garrulus*, *Colaptes auratus* and other eastern birds, while the eastern pike (*Esox estor*) reaches to tide water in the Yukon. In fact the whole fauna has strongly marked Canadian characteristics, which are lost when we pass south of the Alaskan Range.

III. BOTANY.

1. The Rocky Mountain Alpine Region. By C. C. Parry, of Washington, D. C.

The wooded belt of coniferous trees that, with irregular local interruptions, clothes the Rocky Mountain slopes, commences by a somewhat scattering growth near their base, at an average elevation of six thousand feet above the sea. This belt acquires its densest growth, and exhibits the greatest number of distinct species, between seven thousand and nine thousand feet elevation, and terminates by an abrupt well marked line at an average height of eleven thousand three hundred feet.

These plainly recognized features are readily explained by reference to the corresponding climatic conditions here exhibited. Thus the growth is most dense and varied where the exposures present a suitable condensing surface, and where there is the greatest and most regular amount of aqueous precipitation, caused by a mingling of the cool descending currents of air from the higher elevations meeting the warm ascending currents charged with moisture from the heated plains below; at this irregular point of junction summer rains and dews are frequent, and the conditions for arborescent growth are most favorable. At still higher elevations the actual limit of tree growth is determined by conditions of temperature, which satisfactorily explain the peculiar features of vegetation here met with.

Most noticable of these is the singular abruptness, by which this limit of upright tree growth is here marked. You are struggling through a tangled maze of fallen timber and dense underbrush, overshadowed by tall trees, with spreading roots bedded in a saturated spongy soil, when suddenly, without any sensible dwarfing of intermediate forms, you come upon open spaces, where stunted trees fantastically gnarled and twisted, with depressed flattened summits, offer little obstruction to the open view above. Through these obstructions, stepping on the very tops of matted trees which a few rods below rear their pointed spires to a height of thirty to forty feet, you come upon the bare alpine slopes, which continue with variously interrupted rocky exposures to the dividing ridge two thousand to twenty-five hundred feet higher.

In the absence of any continuous meteorological observations at or above the timber line, the most satisfactory explanation of the peculiar features here presented is this: The so-called timber line marks the extreme point of *minimum* winter temperature below which no exposed phenogamous vegetation can exist. All that survives above this point does so by submitting to a winter burial of snow, beneath which protecting cover it is enabled to maintain its torpid existence. The early autumnal fall of snow commences in the latter part of September and receives constant additions through the fall and winter months, during which it retains its light feathery texture, and is not sensibly wasted by melting till the clear lengthening days of early summer dissolve them rapidly, giving origin to the dashing streams that pour down the upper valleys.

It is the pressure of this accumulating weight of snow that gives the fantastic shape to the tree vegetation, that struggles for existence above the well marked timber line, and we can readily note instances, here and there, where from some peculiar condition of wind, or a limited amount of winter-snow in particular seasons, points and patches of dwarfed tree growth being left unprotected, have been blasted and destroyed. Otherwise we can observe still more frequently where ambitious upper branches projecting into the sunlight of this Arctic winter, have been nipped and killed. In these unmistakable signs of the struggle for vegetable existence are also exhibited some of the most peculiar and marked features of the Alpine scenery. This dwarfed tree growth, persisting above the timber line, is as we might naturally suppose confined to sheltered valleys, or on the lea-side of abrupt rocks, where the drifted snow lies heaviest. The point of greatest snow accumulation is mainly determined by the shelter afforded along the upper line of the timber growth, at which locations the snow drifting from the bare spaces above is lodged, hence early in the thawing season, these locations offer the principle obstructions to travel, presenting treacherous fields of snow, often overarching rushing torrents; here also the vegetation is longest delayed, and is comparatively meagre. It is on the more open exposures above that the alpine flora offers its greatest variety, and most attractive features, and through a brief flowering period, extending from June to September, presents a succession of forms and colors, attractive to the eye of a naturalist, and such as is nowhere else so comprehensively exhibited. As these alpine plants owe their existence to the protection afforded by winter snow, they naturally include a number of species that also flourish at lower elevations. Thus in the accompanying list of alpine plants, out of *one hundred and forty-two* species, I note fifty-six as exclusively confined to the alpine exposures. The usual characters of alpine plants here, as elsewhere exhibited, consist in a dwarfed habit of growth, late period of flowering and early seeding, the forms being almost exclusively perennial.

Of Phenogamous plants persisting to the highest elevations, reaching to fourteen thousand feet and upwards, we may enumerate the following: *Thlaspi cochleariforme*, *Claytonia megarrhiza*, *Trifolium nanum*, *Oxytropis arctica*, *Saxifraga serpyllifolia*, *Androsace chamæjasme*, *Chionophila Jamesii*, *Eritrichium aretioides*, *Polemonium confertum*, *Gentiana frigida*, *Salix reticulata*, *Lloydia serotina*, *Luzula spicata*, *Carex incurva*, *Poa arctica*.

Of the thirty-four natural orders represented in the alpine flora, thirty-one belong to *Phenogamous* plants, the remaining three include the higher orders of *Cryptogams*, of the latter, Ferns are represented by a single species, not exclusively alpine (*Cryptogramme acrostichoides* R. Br.). Mosses are more numerously represented, but are still comparatively rare, while Lichens are most abundant and afford the greatest number of species.

Of the Phenogamous orders twenty-seven belong to *Dicotyledons*, four to *Monocotyledons*. Of these the natural order, *Compositæ*, comprises the largest number of species, viz.: twenty-four included in thirteen genera; *Ranunculaceæ* has five genera, seven species; *Cruciferæ*, five genera, six species; *Caryophyllaceæ*, five genera, six species; *Leguminoseæ*, two genera, four species; *Rosaceæ*, four genera, five species; *Saxifragaceæ*, two genera, nine species; *Primulaceæ*, two genera, four species; *Scrofulariaceæ*, six genera, ten species; *Gentianaceæ*, two genera, six species; *Salicaceæ*, one genus, four species; *Coniferеæ*, three genera, five species; *Juncaceæ*, two genera, seven species; *Cyperaceæ*, one genus, four species; *Gramineæ*, five genera, nine species. Of large families entirely unrepresented, we may note *Solanaceæ*, *Labiateæ*.

The superficial extent of these bare alpine exposures can only be approximately estimated in the absence of any exact topographical measurements. Taking the main mountain mass extending through Colorado Territory, or between 37°, and 41°, north latitude, including the high offsets and detached peaks, rising above eleven thousand feet, it would be safe to allow an average width of five miles, for the entire distance, in a straight line, representing in round numbers an area of from twelve hundred to fifteen hundred square miles. Throughout this extent there is great uniformity in the vegetation presented, though agreeably varied by the different exposures or conditions of soil and moisture. Wherever the peculiar texture of the underlying rock has favored disintegration, and the accumulation of soil, a rich alpine sward is presented, made up of densely matted grasses, carices, and plants adapted to pasturage. Here the mountain sheep, the elk, and the Rocky Mountain goat, graze during the summer months, and the mountain ptermigan, and dusky grouse feed and rear their young. When once made accessible it will, no doubt, afford a favorite resort for summer pasturage, and may eventually yield choice dairy products equalling those of the Swiss Alps, or produce delicate fibrous tissues, rivalling those of the looms of Cashmere.

As a sanitary retreat during the summer months it is unexcelled in the purity and coolness of its atmosphere, the clearness of its flowing streams, and its picturesque extended views. There are no elevated points that cannot be safely ascended, and dangers from snow avalanches, or land slips, are so rare as not to be taken into consideration. Of the high culminating points met with in the district under review, including Long's peak on the north, and the Sierra Blanca on the south, there is a remarkable uniformity in the average elevation; all as far as accurately measured rising above fourteen thousand feet. Gray's peak in the dividing ridge, which is now a point of common summer resort, so far carries the palm in an elevation of fourteen thousand two hundred and fifty-one feet. Its associate peak (which it is most earnestly hoped may bear the appropriate name first proposed, of *Torrey's peak*, in commemoration of the early botanical labors of our veteran American botanist) is thought to be somewhat higher, an interesting point which will no doubt be determined by Professor Whitney in his present summer's exploration of that region.

In the accompanying list of alpine plants, published some years since in the Transactions of the St. Louis Academy of Science, I confine the term "alpine" to such plants as are met with on the bald exposures above the timber line; by a (*) prefixed I would indicate those species which are exclusively confined to such localities, while others not thus marked, are also met with at lower elevations.

The subjoined localities, whenever given, denote that the species referred to, is not peculiar to the Rocky Mountains, but is also met with in the different regions there named. *Eu.* indicating Europe, and *As.* Asia.

The concluding summary embodies the general results of my observations in the Rocky Mountain alpine district.

Rocky Mountain Alpine Plants.

Ranunculaceæ.—Anemone narcissiflora, L. *Eu. As.;* A. Nuttalliana, D.C.; Ranunculus Eschscholtzii, Schlecht, Greenland; *R. adoneus, Gray; Trollius laxus, Salisb. *Eu.;* Caltha leptosepala, D.C.; Aquilegia brevistyla, Hook.

Cruciferæ.—Cardamine cordifolia, Gray; Erysimum pumilum, Nutt; *Draba crassifolia, Graham; D. alpina, L. *Eu.;* *Smelowskia calycina, C. A. Meyer, *As.;* Thlaspi cochleariforme, D.C. *As.*

Papaveraceæ.—*Papaver alpinum, L. *Eu. As.*

Violaceæ.—Viola biflora, L. *Eu. As.*

Caryophyllaceæ.—Lychnis apetala, L. *Eu. As.;* *Silene acaulis, L. *Eu. As.;* *Paronychia pulvinata, Gray; *Arenaria arctica, Stev.; A. Fendleri, Gray; Cerastium vulgatum, var. Behringianum, Gray.

Portulacaceæ.—* Claytonia megarrhiza, Parry; C. Virginica, L. var.; * Talinum pygmæum, Gray.

Leguminoseæ.—Trifolium dasyphyllum, Torr. & Gr.; * T. nanum, Torr.; * T. Parryi, Gray; * Oxytropis arctica, R. Br.

Rosaceæ.—Sibbaldia procumbens, L. *Eu. As.;* * Dryas octopetala, L. *Eu. As.;* * Geum Rossii, Ser. *As.*; Potentilla fastigiata, Nutt.; * P. nivea, L. *As.*

Onagraceæ.—Epilobium alpinum, L. *Eu.*

Grosulariaceæ.—Ribes lacustre, Poir. var. (R. setosum, Dougl.).

Crasulaceæ.—Sedum rhodanthum, Gray; S. Rhodiola, L. *Eu. As.*

Saxifragaceæ.—Saxifraga nivalis, L. *Eu.;* * S. cernua, L. *Eu. As;* S. controversa, Sternb. *Eu.;* * S. debilis, Engel.; * S. serpyllifolia, Ph.; * S. flagellaris, Willd. *As.*; S. punctata, L. *Eu. As.;* Parnassia fimbriata, Banks; P. parviflora, D.C.

Umbelliferæ.—* Cymopterus alpinus, Gray; Archangelica Gmelini, DC. *As.*

Araliaceæ.—Adoxa moschatellina, L. *Eu. As.*

Compositæ.—* Erigeron uniflorum, L. *Eu. As.;* E. grandiflorum, Hook; Aster glacialis, Nutt.; A. salsuginosus, Richards. *As.;* Solidago virgaurea, L. *Eu. As.;* * Aplopappus pygmæus, Gray; * A. Lyallii, Gray; * Actinella grandiflora, Torr. & Gr.; A. acaulis, Nutt. var.; Chænactis achilleæfolia, Hook & Arn.; Artemesia arctica, Less. *As.;* * A. scopulorum, Gray; Antennaria alpina, Gaertn. *Eu.;* Senecio amplectens, Gray; S. triangularis, Hook; * S. Soldanella, Gray; S. Fremontii, Torr. & Gr.; S. integerrimus, Nutt.; Arnica angustifolia, Vohl. *As.;* A. mollis, Hook; A. latifolia, Bongard; * Cirsium eriocephalum, Gray; Troximon glaucum, var. dasycephalum, Torr. & Gr.; Macrorhynchus troximoides, Torr. & Gr.

Campanulaceæ.—* Campanula uniflora, L. *Eu. As.;* C. rotundifolia, L. *Eu. As.*

Ericaceæ.—Vaccinium myrtillus, L. var. *As.;* V. cæspitosum, Michx. *As.*

Plantaginaceæ.—Plantago eriopoda, Torr.

Primulaceæ.—* Androsace chamæjasme, L. *Eu. As.;* A. septentrionalis, L. *Eu.;* * Primula augustifolia, Torr; P. Parryi, Gray.

Scrophulariaceæ.—Pentstemon glaucus, Graham; * P. Harbourii, Gray; * Chionophila Jamesii, Benth.; Mimulus luteus, L. var. alpinus; * Synthyris alpina, Gray; * Castilleia breviflora, Gray; C. pallida, Kunth. var.; * Pedicularis Parryi, Gray; * P. Sudetica, Willd. *Eu. As.;* P. Grœndlandica, Retz.

Boraginaceæ.—* Eritrichium aretioides, D.C. *As.;* Mertensia alpina, Don.; M. Sibirica, Don. *As.*

Hydrophyllaceæ.—Phacelia sericea, Jacq.

Polemoniaceæ.—Polemonium pulchellum, Bunge. *As.;* * P. confertum, Gray; * Phlox Hoodii, Richardson; Gilia congesta, Hook.

Gentianaceæ.—Gentiana acuta, Michx.; * G. barbellata, Engel; G. prostrata, Hænk. *Eu. As.*; * G. frigida, Hænk.; *Eu. As.;* G. Parryi, Engel.; Swertia perennis, L. *Eu. As.*

Polygonaceæ.—Polygonum bistorta, L. *Eu. As.;* P. viviparum, L. *Eu. As.;* Oxyria digyna, R. Br. *Eu. As.;* Eriogonum flavum, Nutt.

Salicaceæ.—* Salix reticulata, L. *Eu. As.;* * S. glauca, L. *Eu. As.;* S. arctica, R. Br. *Eu. As.;* S. discolor, Willd. *Eu. As.*

Coniferæ.—Abies Engelmanni, Parry; A. grandis, Lindl.; Pinus aristata, Engl.; P. flexilis, James; Juniperus communis, L. *Eu. As.*

Liliaceæ.—Zygadenus glaucus, Nutt. *As.;* * Lloydia serotina, Reich. *Eu. As.*

Juncaceæ—* Luzula spicata, D.C. *Eu. As.;* L. parviflora, DC. *Eu. As.;* Juncus Drumondii, Meyer; * Juncus Hallii, Engel.; * J. Parryi, Engel; * Juncus triglumis, L. *Eu. As.;* * J. castaneus, Sm. *Eu. As.*

Cyperaceæ.—Carex atrata, L. *Eu. As.;* C. rigida, Good.; * C. incurva, Lightf. *Eu.;* * C. filifolia, Nutt.

Gramineæ.—Phleum alpinum, L. *Eu. As.;* * Poa Andina, Nutt.; P. alpina, L. *Eu. As.;* * P. arctica, R. Br.; P. nemoralis, L. *Eu. As.;* Aira cæspitosa, L., var. arctica, Thurb. *Eu.;* Festuca rubra, L. *Eu. As.;* F. ovina, L. *Eu. As.;* Triticum strigosum, Less. *As.*

Filices.—Cryptogramme acrostichoides, R. Br.

SUMMARY.

1. The persistent bodies of snow which, in variable amount at different seasons, are ordinarily met with on the higher elevations of the Rocky Mountains, do not indicate a region above the true snow line, but result from the accumulation of drifted snow, filling up recesses and sheltered depressions to such an extent that the summer sun is not sufficient to melt the deeper portions, which thus remain from year to year, varying in amount according to the quantity of fallen snow, or the character of the succeeding summer season as to its snow-melting power.

2. Hence, we have no constant accumulation of snow forming what is known in the European Alps as *Nèvé*, the pressure of which from the higher elevations gives origin to glaciers.

3. In the absence of glaciers and heavy snow accumulations on mountain slopes, we do not encounter the usual glacier phenomena so often referred to in the European Alps, and only meet occasionally with avalanches due to accidental local causes.

4. The winter snows being of the light character pertaining to the higher regions of the atmosphere, and not subject to condensation by alternate thawing and freezing during the season of their occurrence, are thus peculiarly liable to the transporting movements of the prevailing winds. Hence results an accumulation of snow in the upper valleys, by which these frozen treasures of winter are safely stored away, to be dispensed in fertilizing streams to the lower valleys during the dry warm season, when most required for agricultural or mining purposes.

5. The peculiar alpine vegetation, attaining to elevations of fourteen thousand feet above the sea level, is enabled to maintain its existence by the protection afforded by the ordinary winter snows, and, in the more sheltered and deeply covered valleys, includes plants which flourish also at much lower elevations.

6. The true timber line, everywhere exhibited as a well masked horizontal plane, varying in elevation, according to the degree of latitude or character of exposure, from ten

thousand seven hundred feet to twelve thousand feet above the sea, indicates a limit beyond which the minimum winter temperature is destructive of all exposed phænogamous vegetation, and whatever in the form of tree growth persists above this point, can only do so by being deeply buried in the accumulation of winter snow, which weighing down their branches, gives that distorted growth pecular to such localities.

7. In the accompanying list, comprising one hundred and forty-two species of alpine plants, fifty-six are noted as exclusively alpine, or confined to the bald alpine exposures; eighty-four species, as far as at present known, are peculiar to the Rocky Mountain range, or to Northern America, while the remaining fifty-eight species are common to the European or Asiatic Alps, or to high northern latitudes of both continents.

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2. On the Sexes of the Plants. By Thomas Meehan of Germantown, Penn.

In my paper on "Adnation in Coniferæ," read before you last year, I believe I established the fact that the stronger and more vigorous the axial or stem growth, the greater was the cohesion of the leaves with the stem. By following the same line of observation, I have discovered some facts which seem to me to afford strong probability that similar laws of vigor or vitality, govern the production of sexes in plants.

If we examine Norway spruces when they are in blossom in the spring, we find the male flowers are only borne on the weakest shoots. The female flowers, which ultimately become cones, appear only on the most vigorous branches. As the trees grow, these strong shoots become weaker by the growth of others above shading them, or by the diversion of food to other channels, and gradually as these shoots become weaker, we find them regularly losing the power of producing female flowers. The law in this instance seems very clear, *that with a weakened vitality comes an increased power to bear male flowers*, and that *only under the highest conditions of vegetative vigor* *are female flowers produced.* The arbor-vitæ, the juniper, the pine, in fact all the different genera of coniferæ that I have been able to examine, exhibit the same phenomena, but the larch will afford a particularly interesting illustration. When the shoots of the larch have a vigorous elongating power, the leaves cohere with the stem, only foliaceous awns give the appearance of leaves. But when they lack vigor—lose the power of axial elongation—true leaves, without awns, appear in verticils, at the base of what with more vigor, might have been a shoot. Every one is familiar with these clusters of true leaves on the larch. In the matter of sex an examination of the tree will show the following grades of vigor. First a very vigorous growth on towards maturity, or the age necessary to commence the reproductive processes. The reproductive age is less vigorous. Taking a branch about to bear flowers we find somewhat vigorous side branches, with the usual foliaceous awns. The next year we find some of the buds along these side branches again branch, but the evidently weaker buds, make only spurs with leaf verticils. As these processes go on year after year, the verticils become shaded by the new growths, and get weaker in consequence, and thus in the third year some of the strongest of these verticils commence to bear female flowers, or a few of the weaker male ones. But only in the fourth or fifth year, when vitality in the spur is nearly exhausted do male flowers abundantly appear. The production of male flowers is the expiring effort of life in these larch spurs. They bear male flowers and die.

What is true of Coniferæ seems also to exist in all Monœcious plants. In the *Amentaceæ* the male flowers appear with the first expansion of the leaf buds in spring, as if they were partly formed during the last flickerings of vegetative force the fall before—but a vigorous growth is necessary before the female flowers appear. In *Corylus*, *Carpinus*, *Quercus*, *Juglans*, *Alnus*, and I believe all the common forms of this tribe, we find the female flowers only on the strongest young growths, and only at or near the apex of the first great wave of spring growth, as if it were the culmination of a great vegetative effort which produced them instead of the decline as in the male. Some of these plants have several distinct waves of growth a year, each successively decreasing in vigor. In such cases the female flowers appear at the apex of the first and strongest wave, and not on the apex of the shoot. This beautiful illustration of the connection of vigor and the sexes can be seen particularly in some oaks, and in *Pinus pungens*, *P. mitis*, *P. rigida* and *P. inops*. In the larch and white spruce a second wave will often cause a spur to elongate, late in the growing season, and even cause a shoot to push from the apex of the young cone. It is essential to note these varying waves of vigor, in one season's growth, and that the apex is not always the strongest point. In *Cyperaceæ*, particularly, these waves vary, and thus we find sometimes the male, sometimes the female flowers at the apex of the culm, but always the female in the line of greatest vigor. I do not know of any case where the sexes are separate on the same plant, that extra vigor does not accompany the female, and an evidently weakened vitality the male parts.

Mere vigor, however, though it often indicates healthy vitality, does not always, or alone do so. *Pinus Mugho* seldom attains ten feet high, and its shoots are not near as vigorous as its near relative *Pinus sylvestris*, yet it commences its bearing age by a free and vigorous production of female flowers. But power of endurance is a test of strong vitality, and an alpine form should possess this in a high degree. In its relation to sex this form of vitality—endurance—will also have an interest. The vitality of a tree is always more or less injured by transplanting. Sometimes it is so injured that leaves never push again, and it always pushes out later than if it had not been moved, and just in proportion to the injury to vitality is the lateness of pushing. Clearly, then, comparative earliness of leafing, is a test of vigorous vitality. Now some Norway spruces push forth earlier than others. There is as much as two weeks difference between them, and it is remarkable that those which push out the earliest—may we not say those which have been most favored by the vital force—are the most productive of female blossoms. Arboriculturists may make good use of this fact. Norway spruces which have a drooping habit, are the heaviest cone bearing forms. No way has hitherto been discovered to detect them until they get to a bearing age. Now it will be seen that the earliest leafers will be the chief cone bearers or weeping trees.

It is not so easy to see the influence of vigor or other forms of high vitality, as affecting the sexes in hermaphrodite as in Monœcious plants, yet here also are some remarkable facts of a similar character. In some flowers the forces which govern the male and female portions respectively, seem nearly equally balanced. Then we have a perfect hermaphrodite—one with the stamens and pistils perfect, each part communicating its influence to the other—a self-fertilizing flower. In many species, however, we notice a tendency to break up this balance. It becomes pistillate or staminate, by the greater development of one force or the other. If the force is in the female direction, it begins by requiring the pollen from some other flower to fertilize itself—if in the male direction, the number of stamens or petals is increased, or the one metamorphosed into the other. The interest for us in this sexual question is to note that just in proportion as the sexes diverge in this manner, in just the same proportion does vigor or some other form of strong vitality accompany the female in the one case, and weakness the male in the other. For instance in the male direction, when the stamens have been turned into petals, or the number of petals increased, growth is never so strong, and life is more endangered. Double camellias, roses, peaches, and other things, have to be grafted on single ones in order to get them to be more vigorous growing plants, and every florist knows how difficult it is to get roots from a double flowered cutting than a single one. Sometimes the male principle, which loves to exhibit itself in the gay coloring of the petals, seems to influence the leaves, and they become colored or variegated, and then also a weakened vitality follows. Variegated box, variegated Euonymus—no variegated plant will grow as freely, endure summer's heat or winter's cold so well, as its regular green leaved form. On the other hand, when the balance goes over in the female direction, we see it characterized by greater vigor than before. It has long been noted that pistillate varieties of strawberries are more prolific than the hermaphrodites, though this is modified according to the disposition of the variety to produce runners, which are really but a form of viviparous flower branches, and thus a legitimate part of the female system. So in *Viola*, where we have many forms of female influence, from the underground stolon, or the creeping runner which propagates without impregnation, to the apetelous flower which mature seeds on the smallest possible quantity of pollen, up to the perfectly formed hermaphrodite flower of spring — all regular and gradual grades of one identical female principle; in contrast with species, which throughout maintain a near connection with the male principle by retaining pure hemaphrodite flowers through their whole stage, we find those possessed of the highest types of vitality which are evidently the most under the laws of female influence.

In a brief paper like this it is not my purpose to introduce more of the facts I have observed than will sustain the probability of the theory I have advanced. I do not wish to urge it for adoption — my object is to excite investigation on the part of other observers, who will, I think, find everywhere about them, that wherever the reproductive forces are at all in operation, it is *the highest types of vitality only which take on the female form.*

I have confined myself to sex in plants, botany being my special study. Do the same laws prevail in the Animal World? I think they do, but this being out of my favorite province, I dare not discuss it, but content myself with the bare suggestion.

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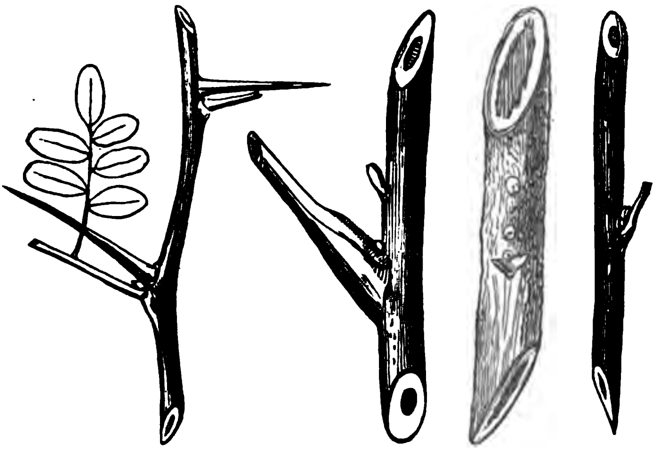
3. ON THE GLANDS OF CASSIA AND ACACIA. By THOMAS MEEHAN, of Germantown, Penn.

DR. ASA GRAY, in his *Manual of Botany*, describes the glands of *Cassia marilandica*, as being towards the base of the petiole. This is true only of the upper leaves. The lower ones have the glands varying in position from near the base up to the lowest pair of Pinnæ. It is clear, from this varying position of the gland, that it is not a normal part of the individual leaf structure. If it were, it would be always in the same position relatively with other parts. It is fair to assume that it is locally an accident. An examination of two allies, *Gleditschia* and *Gymnocladus*, will afford the clue to its real nature.

Of course all know that the spine in *Gleditschia* is an abortive or stunted shoot, and that the true shoot springs when it grows at all, from the bud below. There are therefore two axial buds in this plant, the one above the other. I have discovered that a similar system of buds exist in *Gymnocladus*, only that there are often three in one line, one above another, instead of only two as in *Gleditschia*. These buds rarely push forth into shoots, and hence as you know its name *Gymno-*



Gleditschia triacanthos. Juglans cineria. Gymnocladus canadensis. Itea Virginica.

cladus has been given to it from its naked main branches. It is now worthy of note that the upper bud, the one farthest removed from the axil, is the largest and best developed; and that when a shoot does come it proceeds from it. Also that one or two of the lowest buds are very often below the centre of the axis of the petiole. Turning now to *Gleditschia*, we find that in its two buds, it is the lowest, or what in the other case would be the weakest, which in this instance makes the shoot. It is the upper bud which makes the spine, and I suppose has the least developed vitality. Thus we see in these two allied plants there is no fixed system in the order of axial development; sometimes it is the upper bud, sometimes the lower, which first pushes into growth. We also see by the failure of the upper bud in *Gleditschia* to elongate—by its degradation, to a mere spine—that there is a tendency in all these axial parts to become assimilated into each other.

Turning now to *Cassia Marilandica* and *Acacia julibrissin*, we find that their normal system is to have two buds, one above another, as in the other two; and that the lower bud is nearly opposite the centre of the leaf petiole as before mentioned in *Gymnocladus*; and further that in its early attempts at development it has been absorbed into the tissue of the petiole and borne along with it to a certain extent, and finally become an unwilling part of the leaf stalk.

Trifling as this observation of successive series of buds, one above another, in *Gymnocladus* may seem to be, it may have a very important bearing on our knowledge of the formation of buds. We have been taught that the leaf is the parent of the bud, and an axial bud and leaf are always associated. True, physiologists have noted other buds proceeding from stems and roots; but they have made short work of this mystery by at once deciding that there are two distinct species of buds, and they have termed these leafless affairs adventitious buds; but in the case of *Gymnocladus* we see that of the three buds, one above another,—and the upper one in strong shoots, often an inch away from the lower one,—it is this one the farthest away from the leaf axis which is the strongest. If the leaf exerted any influence, the bud nearest to the leaf axis should derive the most benefit; and further we see in Cassia that instead of the leaf aiding in the development of the bud, it is the direct agent in arresting its growth, and is no doubt also the agent in causing the lower buds of *Gymnocladus* to be weaker than the upper ones.

These series of buds have been singularly overlooked by botanical observers, and therefore the unmistakable voice in which they speak to us has remained unheard. We find them in other plants of very different families. Particularly do they exist in the most vigorous forms of Carya, and Juglans, amongst the *Corylaceæ;* and in Ericaceous plants we find them in *Itea virginica*, in which the upper bud the farthest away from the leaf axis is very fully developed.

I hesitate about offering theories so revolutionary as those to which my observations seem occasionally to tend. I hope the reader will not dwell so much on my explanations of the facts as on the facts themselves. Examine them and possibly a much better theory than my own may be evolved. I chiefly desire to call attention to facts which seem to have been overlooked.

NOTE—Since this paper was read Dr. Geo. Engelmann has pointed out to the author another remarkable instance in *Cornus paniculata* L'Her. There are two buds here; the upper one pushes into growth the same season, and continues the axillary growth of the plants; the lower one, next to the axil of the leaf, remains alive for many years, but rarely grows more than enough to always keep it just above the level of the bark. Also Dr. E. points out that there are three buds, one above another, in *Lonicera Xylosteum* Juss. (I find there are *five*, but two do not get through the bark) but the order of vigor is inversed, the strongest being near the base of the leaf. This observation does not affect the deduction that the leaf has no influence in the production of the buds,—that the leaf is a coincidence with, and not a cause of the bud *or buds*,—but this remarkable exception shows that the whole subject is worth deeper research than bestowed by the author in his brief paper.

IV. ETHNOLOGY.

1. On the Distribution of the Native Tribes of Alaska and the Adjacent Territory. By W. H. Dall, of Washington, D. C.

The principal authorities on the Ethnology and Philology of Russian America, are the works of Count Admiral von Wrangell, and H. J. Holmberg. The former, republished from the *Memoirs of the Imperial Academy of Science*, edited by Baer, and with additions and an appendix by Baer and Helmersen, was issued at St. Petersburg in 1839. Holmberg's work first appeared in the *Acts of the Finnland Scientific Society*, and was published at Helsingfors, under the title of an *Ethnographic sketch of the People of Russian America*, in 1855. This embodied all the additions that had been made by the few explorers since Wrangell's time, and fairly represents the knowledge possessed by ethnologists, in regard to the inhabitants of Alaska, up to a very recent date. From personal observation, during several years exploration in that country, I am enabled to correct many errors, and add to the stock of knowledge of the subject, much new information in regard to their distribution; the more important part of which is embodied in this paper. Holmberg* divided the native inhabitants of Alaska into four groups:—1. Thlinkets (Thlinkilthen); 2. Koniags (Konjagen); 3. Tnaina (Thnaina), or Kenaians; 4. Aleutians (Aleuten); and these groups again into others equivalent to their tribal organizations, as follows:—

Group first into:—1. *True Thlinkets*, extending from the Nasse River to Mt. St. Elias; and the 2. *Ugalentze*, whom he describes as visiting Kayak island in the winter, and spending their summers on the banks of the Copper River.

Group second:—1. *True Koniags*, or inhabitants of the island of Koniag, or Kadiak; 2. *Tchugatches*, from Prince William Sound, along the south shore of the peninsula of Aliaska, except the east shore of Cook's inlet; 3. *Aglegmutes*, inhabiting the north side of Aliaska, part of Bristol Bay, and the mouth of the river Nushergak; 4. *Kiataigmutes*, between the last, on the coast; and 5. *Kuskokwigmutes*, on the Kuskoquin River, from Fort Kolmakoff to the sea, and also on the island of Nunivak; followed on the coast by the 6. *Agulmutes;* 7. *Magemutes;* 8. *Kwikhliuagmutes;* and 9. *Kwichpagmutes*, which occupy the delta of the Yukon River, followed by the 10. *Tschnagmutes*, in Norton Sound, and 11. *Pastolikmutes*, at the mouth of the Pastolik River; the 12. *Anlygmutes*, on Golofnin Bay, north of Norton Sound, and lastly the 13. *Mauegmutes*, between Norton Sound and Kotzebue Sound.

Group third:—1. *Yunnakakotana*, on the Nulato and Koyoukuk Rivers; 2. *Yunnachotana*, on the Yukon River; 3. *Inkiliks*, on the Yukon, south of Nulato; 4. *Yugelnutes*, on the lower Yukon, in the Shageluk slough and mouth of the "Innoko" River; 5. *Inkalikluaten*, beyond the "Innoko;" 6. *Tlegonkotana*, on the river "Tlegon;" 7. *The true Kenaians or Tnaina*, on the peninsula of Kenai; 8. *Kolshina*, on the upper Kuskoquim and Atna Rivers.

Group fourth:—1. *Unalashkans*, or Fox Island Aleutians; 2. *Atkans*, or Andreanoff islanders.

*The difficulty of obtaining access to both the above mentioned works is my excuse for quotation.

This classification needs very extensive revision. Holmberg was misled, partly by the exaggerated and unreliable reports of Zagoskin, the first Russian explorer of the Yukon Valley. It must also be remembered that all his information was derived at second hand, and much of it from publications by unscientific persons and ignorant traders.

The inhabitants of Alaska and the adjacent territories may be divided into two great groups; those who belong to the aboriginal American stock, whom we are accustomed to designate as Indians; and those scattered along our northern coasts from Greenland to Berings Strait, and for whom we have as yet no general term, but who have been called Eskimo, Aleutians; and on the Asiatic side of the straits, Tuski and sedentary Chukchees. This last great group I propose to designate as Orarians;* a single term being needed in generalization, and none of those in use being sufficiently comprehensive for the purpose. The Orarians are distinguished by (1) their language of which the dialects in construction and etymology bear a strong resemblance to one another, throughout the group, and differ from the Indian dialects, as strongly; (2) by their distribution; always on islands, or confined to the sea coasts; sometimes entering the mouths of large rivers, as the Yukon, but only ascending them for a short distance, and as a rule, avoiding the wooded country; (3) by their habits, more maritime and adventurous than the Indians; following, hunting and killing, not only the small seal, but also the sea lion, and walrus. Even the great Arctic bowhead whale, frequently succumbs to their preserving efforts; and the harpoon now universally used by whalers, having superseded the old fashioned article, is a copy in steel of the bone and slate weapon which the Eskimo have used for centuries; (4) by their physical characteristics, a light fresh yellow complexion, fine color, broad build; and especially the largely developed coronal ridge, and an obliquity of the arch of the zygoma. I am informed by that eminent craniologist, Dr. Otis of the U. S. Army Medical Museum, in Washington, who has handled perhaps as many aboriginal American crania as any living ethnologist, that the cranial peculiarities, referred to above,

* From *ora*, a coast, in allusion to their invariable coastwise distribution.

are common to all Orarian skulls, and form a ready means of distinguishing them, being only shared by the northern mound-builders, who were, perhaps their ancestors.

They are confined to the coasts and islands of northern America, Greenland and the extreme north-eastern portion of eastern Siberia, near Bering Strait.

They are known to the northern Tinneh or Chippewyan Indians as "Uskeemé," or sorcerers, and a belief exists among all the Indian tribes acquainted with them, that they are possessed of supernatural powers. This belief is not unnatural, when we compare the stupid and indolent Indian, gorging or starving by turns, with the agile Eskimo in his kyak, seldom, at least in the more favored regions of Alaska, without a reserve supply of food in his storehouse; and as much at home on the waves as a seabird.

The tribes of this group in north-west America and north-east Siberia, may be divided into three lesser groups.

1. Eskimo.
2. Aleutians.
3. Tuski.

The Eskimo tribes are scattered along the Arctic coast very sparingly. They call themselves *Innuit* and take for a more specific designation the name of the locality where they live, as *Unalaklik*, changing the termination so as to make an adjective *Unalakligmute*, applicable to a single man or woman, and of which the plural is *Unalakligmunés*. They have also tribal names, indicating the inhabitants of a certain tract of country. The tribal lines are very faint and they intermarry without scruple; although there does not seem to be any system like that of the totems, among the whole of this group. South of Pt. Barrow the following tribes may be distinguished:

1. *Kaviagmutes.*—They inhabit the peninsula between Kotzebue and Norton Sounds, which is called by them Kaviiak.

2. *Okeeogmutes.*—These inhabit the islands of Bering Strait, and perhaps St. Lawrence.

3. *Mahlemutes.*—Inhabiting the neck of land between Kotzebue Sound and Norton Bay; their chief village is *Attenmute* on the divide.

4. *Unaligmutes.*—Comprises those living on the shores of

Norton Sound, and south on the coast to the Yukon-mouth; comprising beside others, Holmberg's Nos. 10 and 11 of his second group. The names which he uses are mere local designations, hardly subtribal in value.

5. *Kwikhpágmutes*, or *Ekogmutes*.—Inhabit the delta of the Yukon and are found some fifty miles into the interior where the delta begins. They are called *Premorska* by the Russians, meaning "people by the sea," and take their name from one of the mouths of the Yukon, which is called the Kwikhpák. Those living on the Kusilvak mouth are known as Kusilvágmutes.

6. *Mágemutes*.—Or "mink people," live south-west of the Yukon mouth between it and Cape Romanzoff. The previously mentioned tribes all use the labrets, one on each side, just below the corner of the mouth. The men only wear them. In this tribe, however, they have a different fashion. The women wear two "C" shaped ivory hooks, with the points projecting in front, under the middle of the lower lip. They get their name from the abundance of mink in the region they inhabit, almost to the exclusion of other fur animals.

7. *Agulmutes*.—Occupying the region between the Kuskoquim and Cape Romanzoff, and the island of Nunivak. They are a very shameless and filthy race; nor so ingenious as those on either side of them, except in the matter of carving ivory, in which they excel.

8. *Kuskwógmutes*.—Inhabit the mouth and lower banks of the Kuskoquim River.

9. *Nushergagmutes*.—Inhabit the shores of Bristol Bay, near the mouth of the Nushergak River.

10. *Oglemutes*.—Are found on the east shore of Bristol Bay, south of the last and on the north coast of Aliaska peninsula.

11. *Koniágmutes*.—Occupy the south coast of Aliaska eastward to the sixtieth degree of latitude, the island of *Kadiak* (originally *Koniag*) and the adjacent small islands of the *Koniag* or *Kadiak* archipelago.

12. *Chugachigmutes*.—Are found on the south and east shore of the Kenai peninsula and the shores of Chugach Bay, better known as Prince William Sound.

13. *Ugalakmutes*.—The existence of an Eskimo tribe in

the vicinity of Mount St. Elias, is demonstrated by a vocabulary furnished Mr. Gibbs, by the officers of the Russian American Company. Kayak, the name of a small island, said to be occupied by an Indian tribe during the winter, is evidently of Eskimo extraction. This is the last Eskimo tribe, going south on the north-west coast.

The Aleutians may be divided into two groups, which, however, from the deportation of Aleuts by the R. A. Co., and their arbitrary establishment of villages at one time, and as arbitrary destruction of them at another,—have lost much of their distinctness. They are the

1. *Unalashkans.*—Who inhabit the Fox Islands; principally on Unimak, Unaláshka, Umnak and Akhun, and

2. *Atkans.*—Or Andreanoff islanders, who inhabit the islands of Atka, Amlia, Adák and Attú.

Finally the last group of this race, which has been graphically described by Lieut. Hooper, R. N., and who have been described as sedentary or fishing Chukchees (which name has a numerous variety of spelling) Chukluk, or Namollo, and Tuski. Their language at once distinguishes them from the true Chukchee, or "deer-men," as they call themselves, and their physiognomy is different. They differ from Eskimo (with whom they have been at war since 1630, and perhaps for as many more centuries) in not wearing labrets, and in many respects relating to their mode of life. Their generic name is *Yut*, evidently derived from *Innuit* "people." Those on Seniavine strait call themselves *Chuklukmutes*, but they are so few in number and occupy such a small extent of territory, that it is hardly worth while to do more than adopt the general name of

1. *Tuski.*—Proposed by Lieut. Hooper. They extend along the shores of the country between Anadyr Gulf and Kuluchinskaia Bay and Bering Strait.

The Indians are not so easy to group, without more division than perhaps is justified, by our present state of knowledge of some of the tribes. So far as Alaska and the adjacent American territories are concerned, the Thlinkets from one very distinct group, and many points of resemblance seem to suggest that the Ingaliks and Koyukuns, as well as the Atna or Copper River tribes, and the Indians of Kenai should form, with a subdivision for the Kutchin tribes, another; while the Haidahs of the extreme southern part of the Alexander Archipelago, belong with those of Queen Charlotte's Islands to still another group. There are several extra-limital groups requiring notice. Commencing at the south on the coast, we have south of our boundary on the mainland, the *Chimsyans;* and on Queen Charlotte's Islands the Haidahs, who extend across Dixon's entrance, and have several villages on our islands. They are more properly

1. *Kygáni.*—And are only found at this point in our territory.

We next find the Thlinkets inhabiting the Alexander Archipelago and adjacent shores of the mainland. *Kwan* with them signifies "people," and is affixed to the local designation. Of some of the tribes in this and the next group, we only know the Russian names which may or may not, be those by which they call themselves. The Thlinkets may be divided as follows:

1. *Sitka-kwan.*—Occupying Baranoff, and the adjacent islands of the archipelago and having their principal village near Sitka.

2. *Stakhin-kwan,*—Or Indians inhabiting the mouth of the Stikine River and the adjacent coast.

3. "*Yakutats.*"—Or residents in the vicinity of Bering or Yakutat Bay. They are allied by their language to the two previously mentioned tribes, but little is known of them.

We now reach the southernmost point of appearance on the coast of the western Tinnéh tribes, which may be separated into three general groups; those calling themselves *Kutchin* (people), and those who designate themselves *Tinnéh* or *Tahna,* with the same significance. The first of the Tinneh tribes belongs to the latter group, using the word *táhna,* as do most of those near the coast, while those on the upper Yukon and interior are "Kutchin," while those still farther east on the Mackenzie are *Tinneh.* In this general list I shall regard them as a whole.

1. "*Ugalensé.*"—This is the name used by Russian and German authorities to designate a tribe that has its winter quarters on Kayak Island, and resides on the lower banks of the Atna or Copper River in summer. These are referred to

the Thlinket family by Holmberg, but perhaps belong rather with the Tinneh.

2. "*Ah-tená.*"—Living on the upper Copper River, not to be confounded with the Kutchin tribes of the upper Yukon who visit the head-waters of the Copper River to trade, and are called *Kolchina* or *Kolshina* by the Russians; who apply that term to many tribes of whom they know very little.

3. "*Kenai-tená.*"—*Thnaina* on Tenahna of Holmberg, inhabit the shores of Cook's inlet and the country, to the Alaskan Mountains.

4. *Kaiyuh-khotaná.*—(Lowlanders) or Ingaliks. This great tribe speaks essentially one dialect and includes Nos. 3, 4, 5, 6, 7 and 8 of Holmberg's third group of Thnaina. The "Innoko" River is really the Shageluk, and the "Tlegon" "Tatsheg-no," etc., are mythical streams, running through a country which has never been penetrated by white men or Russian creoles, and reported by the Indians to be nearly destitute of fish and game, and hence uninhabited.

5. *Koyúkokhotáná.*—Or Koyukun Indians, inhabit the region north of the great bend of the Yukon on the Koyukuk River. The Nulato Indians, whose language bore more resemblance to the Koyukun dialect than any other Ingalik branch, were exterminated by the Koyukuns in 1851, with the exception of a few children.

6. *Unakhotána.*—(Far off people) inhabit the banks of the Yukon above Koyoukuk Mountain, to the mouth of the Tananah River. There are very few villages, and these, as well as all the Kutchin and Ingalik tribes, living on the river, call themselves *Yukonikhotana* or "men of the Yukon."

7. *Kutchá kutchin* (Loucheux).—Inhabit the country near the junction of the Rat River and the Yukon. These and the following tribes are migratory, following the deer and pitching their lodges anywhere; while the Ingaliks and Koyukuns have well built permanent houses, which they occupy at least for a part of the year.

8. *Tenan kutchin.*—(Mountain men) are found on the banks of the Tananah River, which has never been explored by white men. They come, however, to trade with both Russians and Hudson Bay traders at *Nuklakahyèt.*

9. *Natché kutchin.*—(Wanderers) north of the Yukon and Rat Rivers about longitude 144° W.

10. *Vunta kutchin.*—(Loucheux) on the Rat River farther up, in the H. B. Territory.

11. *Tuk-kuth kutchin.*—(Rat people) occupy the headwaters of the Rat River; on the other hand going up the Yukon.

12. *Han kutchin.*—(Wood people) or Gens de Bois, about two hundred miles above Fort Yukon on the Yukon and beyond them.

13. *Tutchon Kutchin.*—(Crow people) or Gens des Foux on the Yukon, nearly to the site of Fort Selkirk, at the junction of the Lewis River and the Pelly, and finally we find at the headwaters of the Yukon the

14. *Abba-to-tenah.*—Or *Nehaunee* Indians, who are found along the coast range, and parallel with it; and by crossing the same we reach our starting point again among the Stikine Thlinkets.

The limits of this paper will not admit of an elaborate description of the several tribes, but a few remarks of a general nature may not be entirely out of place. The accompanying vocabularies will show more clearly than pages of argument or explanation, the relations which exist between the several dialects. These are only given as specimens for comparison, the bulk of material being in preparation for publication.

It is to be hoped that measures will be taken at once to prevent the utter loss of the traditions and ancient religious rites of the Aleuts and Koniagemutes. These rites were put down eighty years ago, by the Russian missionaries, almost literally with fire and sword. At the same moment that the traders deprived them of their liberty, in order that they might be forced to hunt fur animals for the benefit of the Russians, the priests fired with ardor, and the hopes of promotion in the church, burned their idols, and destroyed, wherever possible, the gorgeous paraphernalia in which the mysterious rites of their ancient religion were performed. These rites were secretly kept up for forty years, but were at last totally suppressed, and the only relics remaining are a few decayed, yet still curious masks, which were placed with the dead, whom the priests did not attempt to disturb. Had they preserved an account of the religion they destroyed we might forgive them their iconoclasm, but their records only contained lying reports of immense numbers of converts to christianity; which reports were so astounding in their exaggeration that, says the Russian historian, Tikhmenief, "they were received with doubt," at St. Petersburg.

The only means by which any part of these traditions can be preserved, is by obtaining them from a few old men who witnessed in their youth the ceremonies referred to, and have not to this day become emancipated from the attendant superstitions. These old men will soon pass away, and if no steps are taken to prevent it, all knowledge of the ancient Aleutian customs with them. My own opportunities did not allow of my obtaining the desired information.

The Eskimo of Norton Sound, the Yukon-mouth and Kotzebue Sound are fine athletic men, many of them six feet in height, and averaging, I should say, as tall as any civilized race. They are as ingenious, as honest and industrious as the majority of white men, and very far superior to any Indian tribe in the territory. They are great eaters, but no more so than the Indians, and they are by far the cleaner of the two. They fall victims to the use of liquor whenever they can obtain it, which is the only obstacle between them and the hope of ultimate civilization. At no point does there seem to be any intercourse between the Eskimo and Indians except in the way of trade. They never intermarry, and in trading, use a sort of jargon, neither Indian nor Eskimo. Few words, as far as I have been able to find out, are common to the two languages, except *kweéñyuk* (pipe), which the Indians borrowed from the Eskimo, who were the first to obtain and use tobacco, and *tenékuh* (moose), an Indian word which is used by the Eskimo, as they have no moose and hence no word for it in their country; and a few evidently similar cases.

The Indians who live in the more mountainous regions, and hunt the deer, are active, courageous and prone to war. Those on the other hand who live on the moose, which frequents the lowlands, are comparatively peaceful; while those on the lower Yukon whose diet is almost exclusively fish, are the lowest, most degraded and filthy of all.

have a sparce growth of hair on the chin. The adults generally wear in the perforated ears and lips pieces of wood, shaped

INDIAN.

TINNEH.		(Western.)	
KUTCHA KUTCHIN.	UNAKHOTANA.	KAIYUHKHOTANA. ** (Ulukuk River.)	(Kaiyuh River.)
Tinjee.	Tenáh.	Tenáhyou.	Tenáhyu.
Trin'-joh.	Solt'án.	Sultána.	T'sult.
At.	Ote.	Ote.	Ote.
T'tsèe-ah.	Keé-os.	Yukee-óza.	Sak'heé.
Nee-chit.	Sol'tanyóza.	Kohtlyózah.	——
Kāh-keh'.	Tyóne.	Tyóne.	Ty'one.
Chu.	Too.	Too.	Too.
Zah.	Nutāhgah.	Nutáhgah.	Nutáhgah.
T'tūn.	T'tūn.	T'hūn.	T'han.
T'sin.	At'khún.	At'khún.	At'kúneh.
Nūn.	——	——	Noo.
N'jūh.	D'ash.	——	——
Ko.	Khūn.	Tahkoóna.	Tahkún'.
Vut-zaih.	Mitzi'kh.	Anóyah.	Anóyh'.
T'thluk.	Mahmúh.	Neliyúh.	T'ht'kakh'.
Et'sih.	T'lahkadóna.	Klah-kadóna.	T'kadóne.
Nirzi'h.	Nahzoón.	Neezoón.	Nezroón.
Nizikwah.	T'sohklāhka.	Tsat'kláhka.	S'talágha.
Choh.	N'koh.	N'koh.	N'tsooh.
T'sul.	Nookatzáh.	Nookootsáh.	Nookóojah.
Needhá.	Aleebúh.	At'kleebáh.	Honalikóh.
Nih'kudh.	Azū'.	Azū.	Atzū.
Sih.	Sih.	Sih.	Sih.
Nun.	N'neh.	N'neh.	N'neh.
Yahtun.	M'muh.	M'mh.	M'mh'.
A-há.	Hoh.	Hoh.	Ah.
No-kwáh.	N'tahgúh.	N'ty'ahoh.	N'tah.
Chithluk.	Kaythluket.	Kaythlukéh.	Kaythlukéh.
Nikai.	N'táyuhkeh.	N'tay'keh.	N'táykhneh.
Tie ik.	Tonkáh.	Tókah.	Tókhneh.
Tahng.	Tinkée.	Ténikeh.	Ténikhneh.
Cheetlukū'nli.	Ketudsinala.	Ketúdnala.	Kétsinala.
Tihkeetieék.	Tonankaythluket.	Tenankáyt luka.	Tonankáythluket.
Etseh detsenekai.	Tonanotáyukeh.	Tenanotáykeh.	Tonanotáykeh.
Tikeetang.	Nihkahdinkéh.	Nilhtadinkeh.	Tl'kád'nkay.
Tenchudnekóhkwa.	Kaythluketkulyeh.	Kaytlukukúlla.	Nikoználakáythlukehkúlla.
Chithlúkehotein.	Neekoznala.	Nikognála.	Nikoznarita.
Kennicott.	Dall.	Dall.	Dall.

** Widely separated branches of the great Ingalik tribe.

Yukon whose diet is almost exclusively fish, are the lowest, most degraded and filthy of all.

A belief in shamanism is common to all, both Indian and Eskimo. The system of totems, according to Mr. Ross, exists among the Kutchin tribes, but is falling into disuse. I found no traces of it among the Ingaliks or Koyukuns. It is in vogue among the Thlinkets, who call it "*lux-pa-té-utk*," and distinguish four totems, the crow, wolf, whale and eagle. The Kutchin or Loucheux distinguish only three. The Eskimo have nothing of the kind. Many of the tribes enumerated are too little known to say whether they have adopted it or not. Few of them are dangerous, fewer still openly hostile to the whites.

In the following vocabularies a number of words have been chosen which would be suited for purposes of comparison. Of the thirty-four tribes enumerated we have vocabularies of only twenty-two and many of these are extremely limited. Of these I am indebted to Mr. George Gibbs for the use of five, from his unrivalled collection, viz.: the *Yakutat*, *Nushergagmute*, St. Elias Eskimo, *Kygani* and *Stakhinkwan*.

The *Kutcha kutchin* was obtained by the late lamented Robert Kennicott.

The *Tenan kutchin*, *Unakhotana*, *Kaiyuhkhotana*, *Yukonikhotana*, *Ekogmute*, *Unaligmute*, *Mahlemute* and *Kaviagemute*, were obtained by myself.

The remainder are from the well known works of Baer, Lisiansky, Wrangell, Saur and Egede.

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2. On the Botocudos of Brazil. By Charles F. Hartt, of Ithaca, N. Y.

(ABSTRACT.)

He spoke of the origin of the name Botocudos, described them of middling height, stout in body, but thin and generally slight in the extremities. They have about the color of light mulattoes; eyes generally dark, rarely blue, cheek bones not very prominent. They generally pull out the beard but some have a sparce growth of hair on the chin. The adults generally wear in the perforated ears and lips pieces of wood, shaped like a bottle cork. The custom is not kept up at present, few of the children having the perforations. The pressure of this plug generally causes the loss of the front teeth, which are pushed out of place. Mr. Hartt gave a graphic description of the appearance of the deformity thus caused.

These Indians sometimes paint not only the face but the body with annatto and a black vegetable dye, but they usually go naked and unpainted. They carry long bows, often exceedingly hard to bend, using arrows of different kinds for hunting and in war. Before the settlement of America by the whites they used cutting instruments of stone. The several tribes are governed by chiefs chosen for their strength and stature. Professor Hartt described their food as consisting of wild animals, including monkeys, lizards, and snakes; they are very fond of the larvæ of beetles found in decayed wood. Bananas, honey, ants, etc., are also used for food. They obtain fire by twirling a dry stick in a small hollow in another. They have only one wife, who is treated very brutally. The women are almost slaves, carrying the burdens and doing all the hard work. Children are generally treated kindly. Their religious ideas are very dubious, they believe in bad spirits, great and small; but they appear to have no idea of a good God. The dead are buried in the wigwam or near it, and the camp is generally deserted. The corpse is buried horizontally without anything in the grave with it, and a fire is sometimes built over the grave to keep off bad spirits. They are strongly suspected of cannibalism. They have very monotonous dances of which they are very fond. The Brazilians have in past years hunted them like dogs and this destruction, with the effects of rum, has almost exterminated the race. The Botocudos are now confined to the forest between the Rio Doce and the Rio Pardo. They resist civilization and christianity, and are sunk in the lowest barbarism.

The speaker gave a minute description of their language, which is simple and almost without inflections.

TITLES

OF

COMMUNICATIONS.*

A. MATHEMATICS, PHYSICS AND CHEMISTRY.

1. On the Determination of the Mechanical Equivalent of Heat by means of the modern ice and cooling Machines. By P. H. Van der Weyde.

2. The Spectral Bands considered as harmonies of one or more fundamental longer waves, laying beyond in the invisible calorific rays. By P. H. Van der Weyde.

3. Atomic Volume and Atomic Distances of the Crystallized A B_3 C. By Gustavus Hinrichs.

4. On the Manufacture and Uses of Amalgams of the Alpali-Metals. By Henry Wurtz.

5. On the Influences which rule Natural Forces. By Henry Wurtz.

6. On the Nature of Metallicity. By Henry Wurtz.

7. On the Conservation of Force.. By H. F. Walling.

8. Elemental Fibres, illustrated by models. By H. F. Walling.

9. The Classification of the Elements of Matter. By Charles A. Seely.

* The following papers were also read: of some, no copy has been received for publication; of others, it was voted that the title only should be printed. No notice, *even by title*, is taken of articles not approved.

10. The Volumetric determination of Carbonic Acid. By Charles A. Seely.

11. On the source of the color of chlorophyle, and the reduction of carbonic acid in plants by phosphate of protoxide of iron, as the first step in the production of organic bodies containing carbon. By E. N. Horsford.

12. On the relations of waste and repair to the metamorphoses of the phosphates. By E. N. Horsford.

13. The source of Salt-water at the bottom of Mystic Pond. By E. N. Horsford.

14. On a New Method of producing by the electric spark figures similar to those of Lichtenberg. By E. W. Blake.

15. On the Ethereal Medium. By Samuel D. Tillman.

16. On a Self-recording Psychrometer. By Samuel Tillman.

17. Some Results of the Discussion of the Boston Dry Dock Tide Observations. By William Ferrel.

18. On the Velocity of the Electric Current over Telegraph Wires. By G. W. Hough.

19. On the Total Eclipse of Aug. 7. By G. W. Hough.

20. Notes on the Chemistry of Copper. By T. Sterry Hunt.

21. New Devices for Producing and Managing Intense Heat. By Charles A. Seely.

22. Formula for Computing the time of Axial Rotation of any Primary Planet, its Density, and the Relative Force of Gravity at its surface, being given. By Ira Wanzer.

23. An Improved Method of Observing Meteoric Showers. By David Murray.

24. Hints towards an Explanation of the Corona in Solar Eclipses. By David Murray.

25. PLANETARY INFLUENCE ON RAINFALL AND TEMPERATURE. By PLINY E. CHASE.

26. ON THE USE OF THE THERMOMETER TO DETERMINE THE PERIOD OF SOLAR ROTATION. By PLINY E. CHASE.

27. REMARKABLE CASE OF FREEZING FRESH WATER PIPES IN SALT WATER. By W. W. WHEILDEN.

28. ON THE GROUPING OF AEROLITES. By J. E. OLIVER.

29. LONGEVITY OF AMERICAN SHIPS, WITH THE APPROXIMATE LAW OF THEIR LOSS OR DECAY. By E. B. ELLIOTT.

30. ON THE VALUES OF THE STANDARD MONETARY UNITS IN WHICH SECURITIES OF THE UNITED STATES ARE QUOTED IN CERTAIN COMMERCIAL CENTRES OF EUROPE. By E. B. ELLIOTT.

31. ON SOME FURTHER EVIDENCE OF THE EXISTENCE OF A SYSTEM OF ARCTIC WINDS. By JAMES H. COFFIN.

32. ON THE IMPERFECT WHITENESS OF SNOW. By J. E. OLIVER.

33. ON THE PRESENT CONDITION OF LIGHT HOUSE ILLUMINATION IN THE UNITED STATES. By JOSEPH HENRY.

34. ON THE SOLAR ECLIPSE, AND THE OUTLINES OF THE CORONA AS OBSERVED AT DES MOINES. By THOMAS BASSNETT.

35. CHEMICAL AND MECHANICAL MEANS AS A PROTECTION OF LIFE AND PROPERTY FROM FIRE ON RAILWAY CARS. By J. F. BOYNTON.

36. THE LAW OF ACCIDENTS. By G. A. LEAKIN.

37. A NEW METHOD OF RENDERING THE NEEDLE OF A GALVANOMETER DEFINITELY ASTATIC. By M. G. FARMER.

38. SMITH'S VULCANITE ELECTRICAL MACHINE, WITH ATTACHED CONDENSER. By M. G. FARMER.

39. THE AMERICAN COMPOUND TELEGRAPH WIRE, REFERRED TO THE BRITISH ASSOCIATION UNITS OF RESISTENCE. By M. G. FARMER.

40. On an Improved Construction of the Holtz Electrical Machine adapted for the Analysis of the Phenomena of this variety of Machine, and for Classroom use. By R. E. Rogers.

41. On the Total Eclipse of August 7, 1869. By Benjamin Peirce.

42. On Quintuple Algebra. By Benjamin Peirce.

43. The Reactive Force of a mass composed of many non-cohering particles or parts. By J. D. Warner.

44. On the Audible Transmission of Musical Melodies by means of the Electric Telegraph. By P. H. Van der Weyde.

45. Electricity not a self existent fluid, but a mode of motion of matter. By P. H. Van der Weyde.

46. Photographing Objects of Natural Size without a Camera. By T. Gaffield.

47. On the Proper Monetary Unit. By J. F. Holton.

48. On the Abolition of Months. By J. F. Holton.

49. On Oceanic Communication. By W. K. Hopkins.

50. The Numerical Method of Criticism. T. M. Coan.

51. On the Resolution of Microscopic Test Objects. By A. M. Edwards.

52. Some Observations, at Montreal, on the Solar Eclipse with Photographs taken by William Notman. By Charles Smallwood.

53. Some Remarks on an Opaque Illuminator, applied to an Immersion Objective, and an Immersion Objective of Long Focal Distance. By E. Bicknell.

B. NATURAL HISTORY.

1. On the Arrowheads of the American Indians and Aborigines. Illustrated by specimens. By John A. Warder.

2. Mammarian Types. By Samuel J. Wallace.

3. Description of a New Species of Chiton. By William Prescott.

4. On the Non-Fossiliferous Rocks of New England. By N. T. True.

5. Exhibition of a few interesting implements collected by R. W. Haskins from Indian graves on the banks of the Ohio, with special reference to the boring of holes in stone implements. By F. W. Putnam.

6. On Embryonic Characters in American Salamanders. By E. D. Cope.

7. On two new genera of Extinct Cetacea. By E. D. Cope.

8. Experiments in connection with the case of Mon. Groux of Hamburg. By J. Baxter Upham.

9. On the Deposits of Fluviatile and Lacustrine Gold. Henry Wurtz.

10. On the Discovery of the Ammonoosuc Gold Field. By Henry Wurtz.

11. Notes on the Geology of Hoboken. By Henry Wurtz.

12. Studies of the Red Sandstones of New Jersey. By Henry Wurtz.

13. Note upon the Palæotrochis. By Henry Wurtz.

14. On the Early Stages of Brachiopods. By E. S. Morse.

15. Popular Science. By Mrs. Lincoln Phelps.

16. The Ammonoosuc Gold Field in New Hampshire and Vermont. By C. H. Hitchcock.

17. Indian Migrations. In Four Sections. Sec. 1. Physical Geography of North America, with reference to Natural Highways; and Means of Natural Subsistence afforded by its Areas. Sec. 2. Agricultural Subsistence, and the character and extent of Indian Agriculture. Sec. 3. Migrations of Roving and partially Village Indians; deduced from languages, traditions, and known migrations. Sec. 4. Migration of Village Indians; as deduced from the same sources. By L. H. Morgan.

18. Compression as an agent in geological metamorphism, with illustrations of distorted pebbles in conglomerates. By George L. Vose.

19. On the Plumage of Terns. By Miss Grace Anna Lewis.

20. Thoughts on the Structure of the Animal Kingdom. By Miss Grace Anna Lewis.

21. On a Remarkable Locality of Vertebrate Remains in the Tertiary of Nebraska. By O. C. Marsh.

22. Discovery of the Remains of the Horse among the Ancient Ruins of Central America. By O. C. Marsh.

23. On some New Mosasauroid Reptiles from the Greensand of New Jersey. By O. C. Marsh.

24. On the Metamorphosis of Siredon into Amblystoma. By O. C. Marsh.

25. On the Geology of Venezuela. By R. P. Stevens.

26. Observations on a New Genus of Polyzoa. By Alpheus Hyatt.

27. Remarks on Trichina spiralis. By J. Baker Edwards.

28. On the Accent of Speech and its relation to the vital functions. By J. Stanley Grimes.

29. On the Homologies of the Palæchinidæ. By Alexander E. R. Agassiz.

30. Notice of Fossils from Table Mountain, California. By W. P. Blake.

31. Summary of Results of a Late Geological Reconnoissance of Louisiana. By E. N. Hilgard.

32. Hints on the Stratigraphy of Palæozoic Rocks of Vermont. By J. B. Perry.

33. Physical Geography among the Aborigines of North America. By N. T. True.

34. On Surface Changes in Maine indicating the length of time since the close of the Quaternary Period. By N. T. True.

35. On Norite or Labradorite Rock. By T. Sterry Hunt.

36. On the Geology of North-eastern America. By T. Sterry Hunt.

37. On Ancient Erosions in the St. Lawrence Valley. By T. Sterry Hunt.

38. Post-Glacial Fossils at Hoboken, N. J. By R. P. Stevens.

39. On the Geology and Physical Geography of a part of the coast of Maine. By John Johnston.

40. On the Distribution of Coal, Iron, and the Precious Metals, in China. By Albert S. Bickmore.

41. On the Arctic Ocean, the Movements of its Waters, Tributaries, and the Approach of the Gulf Stream. By W. W. Wheilden.

42. On Certain Peculiarities in the Distribution of Marine Life on the Sea-bottom of the Bay of Fundy. By A. E. Verrill.

43. On the Relations of the Geology of Ohio to that of the adjoining States. By J. S. Newberry.

44. On the Flora and Fauna of the Fresh Water Tertiaries of Oregon and Idaho. By J. S. Newberry.

45. On the Tertiary Flora of Alaska. By J. S. Newberry.

46. On the Raritan Clays of New Jersey. By J. S. Newberry.

47. On New Species of Fishes obtained by Prof. Orton in the valleys of the Maronon and Napo. By Theodore Gill.

48. On the Systematic Relations of the Lamarckian Pterocerae. By Theodore Gill.

49. Preliminary Notice of the Lamellibranchiates of the Upper Helderberg, Hamilton, and Chemung Groups. By James Hall.

50. Results of a late Geological Reconnoissance in Louisiana. By E. W. Hilgard.

51. Note on a Phase in the Reproduction of a Confervaçeous Alga belonging to the genus Œdogonium. By A. M. Edwards.

52. On some points in the Geology of North Carolina. By W. C. Kerr.

53. On the Physical Geography and Geology of Brazil. Charles F. Hartt.

54. On Brazilian Drift. By Charles F. Hartt.

55. The Homologies and General Structural Relations of the Polyzoa. By Alpheus Hyatt.

56. Notice of some New Fossil Plants from Gaspe, discovered by Prof J. W. Dawson. By J. S. Newberry.

57. On the Dyestone Fossil Iron Ore in Pennsylvania. By J. P. Kimball.

58. Observations on the Languages of South America and the Classification of the Indian Nations thereof. By Porter C. Bliss.

59. A Conjectural Explanation of the uses of the Embankments of the Mound Builders. By L. H. Morgan.

60. On the Origin of Muscular and Mental Force. By George F. Barker.

61. The Ainos, or Hairy Men of Yesso, Saghalien, and the Kurile Islands. By A. S. Bickmore.

62. On the Classification of the Diurnal Lepidoptera. By S. H. Scudder.

63. The Morphology of the Abdominal Appendages of Butterflies. By S. H. Scudder.

64. The Value of the Characters drawn from the external Armature of Lepidopterous Larvæ. By S. H. Scudder.

65. The Internal Anatomy of Danais Erippus. By S. H. Scudder.

66. A Classification of the Eggs of Butterflies. By S. H. Scudder.

67. Evidences of high antiquity in the Kjœkkenmœdden Deposits of New England. By E. S. Morse.

68. On Laws of Trade. By E. B. Elliot.

69. On the Productiveness of the Human Race. By J. F. Holton.

70. On Cleaning Guanos so as to obtain the Microscopic Organisms in them. By A. M. Edwards.

71. Some Remarks on the Infusorial Deposits of North America. By A. M. Edwards.

72. Comparison of the Coral Faunæ of the Atlantic and Pacific Coasts of the Isthmus of Darien, as bearing on the supposed former connection between the two Oceans. By A. E. Verrill.

EXECUTIVE PROCEEDINGS

OF

THE SALEM MEETING, 1869.

HISTORY OF THE MEETING.

The Eighteenth Meeting of the American Association for the Advancement of Science was held at Salem, Mass., commencing on Wednesday, August 18, and continuing to Tuesday Evening, August 24.

Two hundred and forty-four names are registered in the book by members who attended this meeting. One hundred and fifty new members were chosen, of whom one hundred and eleven have already signified their acceptance by paying the annual assessment, and, when practicable, signing the constitution. One hundred and sixty-two papers were presented, many of which were read, and some of them discussed at length.

The sessions of the Association were held in the County Court Houses, and in the Vestry and Church of the Tabernacle Society. At about 10 o'clock A.M. on Wednesday the members were called to order by Dr. B. A. Gould, the retiring President, who, in a few appropriate words, introduced the President elect, Col. J. W. Foster. At the request of the Standing Committee, prayer was offered by Rev. E. S. Atwood.

The chairman of the Local Committee, Dr. Henry Wheatland, then introduced the Association to His Honor Mayor Cogswell, of Salem, speaking as follows: —

Mr. Mayor:—Allow me to introduce to you the President and members of the American Association for the Advancement of Science.

This Association dates its origin to 1840, when, on the 2d of April of that year, some eighteen gentlemen, principally connected with the several State Geological Surveys then in progress, met in the hall of the Franklin Institute, Philadelphia, at the request of the members of the New York Survey, and organized an association under the name of "The Association of American Geologists." At the meeting in Boston, in 1842, it was decided to enlarge the objects so as to embrace the collateral branches of natural science, the name being changed so as to read "The Association of American Geologists and Naturalists." At the meeting in 1847, the objects were still farther enlarged so as to include all departments of science. A new organization was effected under the present name. Thus, in the spirit of this enlarged constitution, the Association opened its doors wide for the admission of students in every department of positive science, convinced that the time had come for thus extending its operations. This Association is republican in its organization and migratory in its visits; meetings have been held in various cities of the Union annually, except during the years 1861-65, when they were suspended in consequence of the great crisis through which the country has recently passed, the meeting for 1861 having been appointed for Nashville.

Twenty years since, the day following the adjournment of the Cambridge meeting, the Association visited Salem in the steamer R. B. Forbes, which was placed at its disposal by the kindness of the proprietors, and spent several hours in inspecting the Museum, the rooms of the Institute and other objects of interest. This meeting was long remembered from its many interesting associations.

It is always pleasant at these meetings to witness the assemblage of so many zealous and enthusiastic workers in science, scattered necessarily over a large extent of territory, and kept asunder by distance and the claims of professional duties; laboring amid all the inconveniences of solitude, and isolated from the sympathy and counsel of those engaged in the same glorious enterprise, thus coming together, becoming acquainted with each other's social and scientific worth, comparing notes, and contributing largely to scientific knowledge. May this meeting leave many pleasant associations, and be long remembered as productive of good results, and a lasting friendship between all the members.

Mayor Cogswell responded in the following terms:—

MR. PRESIDENT AND GENTLEMEN OF THE AMERICAN ASSOCIATION FOR THE ADVANCEMENT OF SCIENCE:—

In the name of the people of the city of Salem I have the honor to bid you a hearty and most cordial welcome, and to express to you our hope that this visit may be so pleasant and instructive that you may be induced to repeat it at no distant day. I am charged also with the agreeable duty of tendering to you the hospitalities of the city and the courtesies of her citizens. Coming as you do from all parts of the Continent, bringing with you as you do the results of years of scientific study, meeting as you do to discuss the important questions embraced within the objects of your Association, we feel that your presence in our midst is an honor to our city, and that we shall be benefited by your coming. We hope that along our shores you may find objects of interest and instruction, and that the great study of nature and of nature's God in which you are engaged may be promoted thereby. We delight somewhat in our rocky coast, for we believe it has borne hardy sons. We shall delight to show you everything of interest which we have, while from you we shall expect much of knowledge and information. Here, and in this vicinity, as you know, was first planted that deep-seated, earnest, anxious system of religion which developed or was developed by the Puritans, and which has left its impress wherever the son of the Puritan has trod. Religious as our fathers were, however, material considerations were not neglected, and the port of Salem had the honor of first opening up to the new continent the wealth of the Indies and the fruits which have flowed therefrom. Some of this trade has now gone out from us to build up other places, but the city of Salem, with an energy increasing every day, is now devoting herself to industrial pursuits on her own soil, taking her place, as all eastern cities must, as the manufacturer of what the South and West produce. We have also here some old traditions, I believe, of what took place among our people in those earlier days, of which you probably may have heard. The accomplished gentlemen in charge of our County Court records will be pleased to show you, if you will, some of the original documents relating to those mysterious times, which it may interest you to examine. Whatever would have befallen a convention of progressive scientific men in that day it might perhaps be hard to tell; but I can assure you, gentlemen, that this day your convention will be safe and undisturbed. It is not for me, however, to speak the praises of Salem, but rather to extend to you again its welcome. Welcome to our midst; welcome to the duties of the session; welcome to all the pleasure and entertainment and kindness in our power to grant you! From the West I greet you as the representatives of a mighty power in this country, in whose hands almost alone remains the weal or woe of the still great problem of our form of government! From the South I greet you as the representatives of a section which it is my prayer may soon blossom as the rose, and to whose sunny clime and fertile soil it is my heartfelt wish may soon return a wealth, a happiness, and a peace which shall last for ever and ever. From the North and East I welcome you as neighbors; and from the realms of the illustrious Queen I bid you welcome to the vicinity of the birth-place and the home of that distinguished citizen whose world-wide benefaction your Queen has delighted to honor and acknowledge.

At the close of Mr. Cogswell's remarks, President Foster responded, addressing the Mayor and the Association, in the following words:—

Mr. MAYOR:—As the official organ of the American Association for the Advancement of Science, I express to you my profound thanks for the cordial welcome extended to us on this occasion. It gives us pleasure once more to meet within the limits of this ancient Commonwealth, illustrious in its historical associations, and in those institutions designed for the cultivation of the intellect, and the alleviation of the ills incident to our common humanity.

It is proper, too, that we meet within the limits of this good old town of Salem; for, years ago, as the Mayor has informed you, its merchants, when they sent forth their ships to the East, which returned freighted with

——— "the wealth of Ormus and of Ind,"

were wont to dedicate whatever was rare or curious to the cause of science. This was the origin of the East India Museum, the first institution of the kind, I believe, in the United States. The Essex Institute is another expression of the zeal of your people in the cultivation of those arts which dignify and adorn life. The Peabody Academy of Science is a noble monument to the comprehensive beneficence of its founder, and one among the many of those benefactions which have made his name illustrious in both hemispheres.

The American Association, then, take pride in meeting here. It is an organization, catholic in all its aims and objects. In its ranks are enrolled men representing nearly every profession and every portion of the Union. We here meet on common ground—to compare views, to discuss problems, to eliminate truth, to announce results; and, Mr. Mayor, when we shall have closed our deliberations, I doubt not that each one of us will carry to his home kindly remembrances of the courtesy and hospitality of the good people of Salem.

GENTLEMEN OF THE AMERICAN ASSOCIATION FOR THE ADVANCEMENT OF SCIENCE:—I desire to tender to you my profound thanks for this manifestation of your regard in calling me to preside over your deliberations. It is a position worthy of all aspiration. But the possessor must approach its duties with fear and distrust. It is with these feelings that I present myself before you on this occasion, but I trust that by the exercise of a spirit of strict impartially in the discharge of my duties, I shall win your approbation and support.

The eighteenth annual session of this Association commences under favorable auspices. At its last meeting the attendance was never greater; its treasury never better supplied; and the memoirs, in number, variety, and interest, were never surpassed. There was a spirit of harmony and good fellowship among ourselves, and of zeal and devotion to the cause of science, from which we can draw the happiest auguries.

We have now enrolled in our ranks a class of young men who are better educated, and have had far greater facilities for investigation than were enjoyed by us, their seniors, at their age, and to them we may safely confide the interests of this Association when we shall have rendered up our trust. What to us, at their time of life, was but a glimmering of light, to them is meridian splendor. What to us were but reasonable conjectures, to them are matured results. What we saw as through a glass darkly, they are permitted to see face to face. The verge to which we have attained, after long toil, is to them but the starting point for new explorations into the domain of nature. The future historian in treating of these times will regard this as an age, compared with past ages, when the human mind put forth its most vigorous manifestations, not only in reference to pure science, but also in reference to the practical arts of life. This has been accomplished by a faithful study of the laws of nature, and by acquiring a mastery over physical forces,—thus showing that nature is not like the Delphic Oracle of old which gave forth mysterious utterances and capable of two-fold interpretation, but rather like a beneficent deity, ready to reward each votary who, with all-reverent spirit and all-sentient ear, enters her sanctuary and listens to her teachings.

Many of the Natural Sciences have developed into that beauty and harmony which now characterize them within the lifetime of many of us. Yet what we have accomplished is but the prelude to what shall be accomplished. The Chairman of the Local Committee has referred to the origin of this Association, and has very properly told you that that origin dates back to the year 1840, when less than twenty ardent and devoted men, for the most part engaged in the State Geological Surveys, assembled in the rooms of the Franklin Institute, at Philadelphia, and there organized as the Association of American Geologists. He has traced that organization until it has assumed its present form. I take pleasure in recurring to that early history, because it was an event in American science. The necessity for such an organization was apparent. Geology at that time had hardly assumed the shape of a science. Formations of the same age, and having a wide geographical range, bore different local names; and the entombed organic remains, which have proved the hieroglyphics by which we have been enabled to interpret the physical history of our planet, were then but little known. To compare views, to receive and impart instruction, and fix upon a common nomenclature, was the business of these observers. Each one brought forward the fruits of his observation to contribute to the general fund, —as of old among the Greeks, when a hero died, each warrior brought forward a shield full of earth, and cast upon his remains, until there was erected a mausoleum which rose up a conspicuous landmark against the sky and destined to endure for all time.

The modern student of geology may smile at some of the speculations contained in the earlier proceedings, founded on the supposed persistency of lithological characters in the several formations,—as though the streams the world over were charged with the same sediments; as though the ocean floor everywhere contained the same forms of organic life; and as though volcanoes were simultaneously excited and poured forth the same igneous products; but to their honor be it said that that little band of men, who, unheralded and without parade, thus assembled at Philadelphia, in 1840, were emphatically the fathers of American geology.

Of the original founders time has more than decimated their ranks. Of the survivors some are in this presence and will participate in these proceedings. But I must not speak of the living, however meritorious; my reminiscences are of the dead. And in this connection I would recall the name of Bailey, who, armed with the microscope, brought out results in the organic world almost as wonderful as those of Ehrenberg; of Mather and Vanuxem, and Ducatel, and Locke, who, to patient industry united keen powers of observation, and who have left behind enduring memorials; of Henry D. Rogers, who made some of the grandest generalizations in American geology and whose merits were recognized in both hemispheres; of Houghton, who devised an admirable system of geological survey in connection with the lineal survey of the public domain, and from whom I parted a few hours before he became engulphed in the remorseless waves of Lake Superior;

of Niccolet, a foreigner by birth, impoverished in means, and in feeble health, who, after five years' toil brought out his astronomical survey of the valley of the Upper Mississippi; of Redfield, among the foremost of our observers in meteorology; of A. A. Gould, eminent as a conchologist, and an ardent promoter of all science, who was stricken down by the cholera when returning, after the war, from a meeting of this Association, at which he helped to revive its drooping fortunes; of Johnson, whose report on the comparative evaporative power of our coals may yet be consulted with advantage; of R. C. Taylor, whose work on "The Statistics of Coal" contains a vast range of information which has served as the basis of our subsequent investigation; of Harlan, who, at that time, was our only comparative anatomist; of Morton, among the most brilliant of our scientific men, who died prematurely for his own fame, prematurely for the cause of science; and last but not least, of Hitchcock, the veteran observer, and Silliman, who in his Journal, combined and crystallized American science. Hitchcock and Silliman went down to the grave ripe in years and crowned with honors; and when I see before me the sons engaged in the same pursuits which the fathers illustrated and adorned, I am reminded that

"E'en in their ashes live their wonted fires."

I have deemed it proper on this occasion to pay this passing tribute to the memory of these early cultivators of American science. I fear, gentlemen, that I have detained you too long. Again thanking you for your courtesy, I close with the expression of the hope that our deliberations may be harmonious, and that they may tend, in the language which we have incorporated into our title, to "the Advancement of Science."

At the close of the address the Association proceeded to business, and elected Mr. F. W. Putnam, Permanent Secretary *pro tem.* in the absence, in Europe, of Prof. Lovering. Six additional members of the Standing Committee were elected by ballot, according to the requirement of Rule 4 of the Constition. The names of those chosen are printed elsewhere with the names of the other members of that committee.

Later in the session the Association voted to hold its next meeting at Troy, N. Y., beginning on Wednesday, August 3, 1870. The officers elected for the next meeting are: —

Prof. WILLIAM CHAUVENET, of St. Louis, *President;* T. S. HUNT, Esq., of Montreal, *Vice-President;* Prof. C. F. HARTT, of Ithaca, *General Secretary;* Dr. A. L. ELWYN, of Philadelphia, *Treasurer.* Prof. JOSEPH LOVERING was elected *Permanent Secretary* for another term of two years commencing with the Troy Meeting.

On the afternoon of Wednesday, August 18, the members of the Association were present, by invitation, at the dedication of the Peabody Academy of Science. On Friday evening, August 20, the address of Dr. B. A. Gould, the retiring President, was given in the Tabernacle Church. There was a general session at Lyceum Hall on Thursday evening, to witness some physiological experiments by Dr. J. B. Upham and others.

Many of the members in attendance upon this meeting accepted the private hospitality, generously offered by families in the city of Salem. On Saturday, August 21, by invitation of the City Council of Salem, the American Association for the Advancement of Science enjoyed an excursion in the bay on board the steamer Escort, which was gaily decorated for the occasion. The boat, with a company of between four and five hundred ladies and gentlemen on board, proceeded to Minot's Light and afterwards to Fort Warren. Major McConnell, the officer of the day, received the excursionists, who, headed by Mayors Cogswell of Salem and Shurtleff of Boston, entered the fortress gate, marching to the music of the accompanying band. After spending a short time in viewing the various objects of interest in and about the fort, they reëmbarked and proceeded to Nahant; where a dinner, provided by the city authorities of Salem, was eaten in the Maolis Garden. At four o'clock the steamer was again taken and, after a very pleasant sail along the Gloucester, Manchester, and Beverly shores, the party returned to Salem.

On different evenings of the session, receptions were given by W. C. Endicott, Esq., Mrs. Walcott, Dr. G. B. Loring, and the Salem Board of Trade.

RESOLUTIONS ADOPTED.

Resolved, That a committee be appointed to act as the correspondents of the various local committees, and assist them in making arrangements with the different railroads throughout the country for the transportation of the members of the Association to and from the meeting.

Resolved, That a request be addressed by the President of the Association to the Secretary of State of the United States, asking for the influence of the State Department, with the civil authorities of Paraguay, to procure the restoration of the manuscripts of Mr. Porter C. Bliss, relating to the Indian languages of South American, of which he was deprived by order of President Lopez, and which are now supposed to be in his custody.

The following resolution from the Subsection of Archæology and Ethnology was approved by the Association.

Resolved. 1. As the sense of the Subsection of Archæology and Ethnology that explorations for the collection of remains of Aboriginal Art are essential to progress, and that the Section takes this method and occasion to express their interest in, and commendation of, the proposed expedition of Mr. McNiel to Nicaragua. 2. That a copy of this resolution be presented to Mr. McNiel.

Resolutions relating to an International Statistical Congress.

Whereas This Association has been informed that a suggestion has been made on the part of some of the foreign members of the International Statistical Congress in favor of holding one of its sessions in the United States: —

Resolved, That the members of this Association hereby express their cordial approval of this suggestion, and their desire that the International Statistical Congress may decide to make the United States its place of meeting at an early day.

Resolved, That an attested copy of these resolutions be for warded to the presiding officer of the International Statistical Congress, soon to be held at the Hague, in the Netherlands, and to the delegate representing the United States in said Congress.

Resolution in regard to the sale of Proceedings.

Resolved, That the proceeds of all future sales of Proceedings of the Association be reserved and invested by the Treasurer, to form, together with any donations for the purpose, the nucleus of a special fund, the interest of which may, at some future time, be devoted to scientific researches under the direction of the Association.

VOTES OF THANKS.

Resolved, That the thanks of this Association be tendered to the Commissioners of Essex County for the use of their admirably arranged rooms in the Old and New Court Houses in Salem.

Resolved, That the thanks of this Association are due to the Local Committee, who, by their assiduity in providing for our reception and accommodation, have rendered this session one of unusual enjoyment and long to be remembered.

Resolved, That the hearty thanks of this Association be presented to the citizens of Salem, who have so kindly and hospitably entertained the members, during the present Session.

Resolved, That the thanks of the Association are especially due to the Essex Institute, to the Trustees of the Peabody Academy of Sciences, and to the Board of Trade of the City of Salem, for their untiring exertions in providing for the accommodation of the meetings of the Association and facilitating the transaction of its business.

Resolved, That the thanks of the Association are due, and are most respectfully offered, to the ladies of Salem, who have so kindly and hospitably taken charge of the refreshment tables, which have been daily spread for our welfare.

Resolved, That the thanks of the Association be tendered to the Proprietors of the Tabernacle Church, for the use of their church and vestry during the present session.

Resolved, That the sincere thanks of the Association are hereby tendered to the Athenæum of Salem, to the Museum of

Comparative Anatomy at Cambridge, to the Authorities of Boston, to the Massachusetts Institute of Technology, to the Boston Natural History Society, to the American Academy of Arts and Sciences, to the Massachusetts Horticultural Society, and to the Boston Music Hall Association, for courtesies shown to this Association, and at the same time the Association begs to express its regret that, by reason of the press of business, it was unable, in all instances, to avail itself of these courtesies.

Resolved, That the Association acknowledges its obligation to the Directors of the following railroads, and especially of the Eastern railroad, for their liberality in granting a reduction of fare to the members attending the present session.

Eastern.
Boston and Albany.
Boston and Providence.
New York, Providence, & Boston.
Boston and Maine.
Portland, Saco and Portsmouth.
Fitchburg.
Vermont and Massachusetts.
Vermont Central and Sullivan.
Connecticut and Passumpsic.
Boston, Lowell and Nashua.
Portland and Kennebec.
Richmond, Fredericksburg and Potomac.
Richmond and Petersburg.
Virginia and Tennessee Southside.
Norfolk and Petersburg.
South Carolina.
Memphis and Charleston.
North Eastern.
Illinois Central.
Great Western.
Wilmington and Manchester.
Nashville and Chattanooga.
Hartford and New Haven.
Terre Haute and Indianapolis.
Marietta and Cincinnati.
Indianapolis, Cincinnati & Lafayette.

A resolution was also passed expressing thanks to the Horse Railroad Companies of Salem for courtesies extended to the members of the Association.

A resolution was also passed expressing high appreciation and commendation by the Association of the Peabody Academy of Science in Salem.

Also a resolution, giving a vote of thanks to the Mayor and City Council of Salem, for the Excursion on Saturday, the 21st of August.

REPORT OF THE PERMANENT SECRETARY.

This report relates to those transactions of business which belong to the interval between the first day of the Chicago meeting [August 5, 1868] and the first day of the Salem meeting [August 18, 1869].

During my absence in Europe, the details of my office have received prompt attention, as heretofore. The Chicago volume has been printed under the direction of Mr. F. W. Putnam, the Director of the Peabody Academy of Science, and distributed by my assistant, Mr. J. W. Harris. Circulars were sent to those members of the Association who were indebted to it for assessments, and the collection of money, in response to these circulars, has been considerable, although many are still largely in arrears.

The financial condition of the Association is as follows:—

Between August 5, 1868, and August 18, 1869, the income of the Association was three thousand and eleven dollars and thirty cents ($3 011.30).

Of this amount, one hundred and eleven dollars accrued from the sale of the printed Proceedings, and the remainder from the admission fee and the annual assessments.

The expenses of the Association, during the same interval, amounted to nineteen hundred and fifty-one dollars and seventy-seven cents ($1951.77), which may be apportioned thus:—

Cost of paper, printing, and binding, for the volume of Chicago Proceedings, and expense of its distribution, ...	$1 243.04
Charges connected with the Chicago meeting, .	$144.35
Salary of the Permanent Secretary, five hundred dollars, ...	$500.00
Circulars, postage, stationery, express, . . .	$64.38

The particular items may be found in the cash account of the Secretary, which is herewith submitted as a part of his report. The balance in the Treasury of the Association on August 18, 1869, is twelve hundred and twenty-five dollars and nine cents ($1 225.09).

JOSEPH LOVERING,
Permanent Secretary.

SALEM, August 18, 1869.

CASH ACCOUNT OF THE

Dr. AMERICAN ASSOCIATION *in*

J. W. Harris's bill as clerk,	$60.85
Stowell's bill for copying,	2.00
Janitor at Chicago,	1.50
Postage Stamps,	9.00
Freight to the Smithsonian Institution,	3.60
Binding Cash Book,	1.00
Printing Circulars,	5.00
Box to and from Chicago,	10.79
Envelopes,	1.38
F. W. Putnam, for postage,	12.21
Guests' bill for reporting,	5.00
Adams' bill for binding Proceedings,	84.78
Postage stamps,	16.00
Carter, for paper and envelopes,	3.00
Ripley, for printing circulars,	6.00
Salary of the Permanent Secretary,	500.00
Paper and printing for the Chicago Proceedings,	1,153.21
Box of Proceedings to Washington,	1.45
Attendance at the Chicago meeting,	75.00
	1,951.77
Balance to next account,	1,225.09
	3,176.86

PERMANENT SECRETARY. Cr.

Account with JOSEPH LOVERING.

Balance from last account,*	$165.56
Assessments (from No. 1 to No. 451 of Cash Books) including the sale of Proceedings, ...	3,011.30
	3,176.86

* This was erroneously printed, in the last volume, as $150.56.

STOCK ACCOUNT OF THE PERMANENT SECRETARY.

Volumes Distributed or Sold, since the report in Vol. XVI.

VOLUMES.	I.	II.	III.	IV.	V.	VI.	VII.	VIII.	IX.	X.	XI.	XII.	XIII.	XIV.	XV.	XVI.	XVII.
Distributed to Members,	1	1	1	2	2			1		1	3	2	7	19	64	226	358
Harvard College,																1	1
Yale College,																1	1
Boston American Academy,																1	1
Boston Nat. Hist. Society,																1	1
Boston Athenæum,																1	1
Boston City Library,																1	1
Conn. Acad. Sciences,																1	1
Brown University,														1	1	1	1
Smithsonian Institution,														1	1	1	1
Providence Athenæum,														1	1	1	1
Foreign Societies,*																58	58
Sold,	6	3	3	7	1	7	1		1		1	2	2	4	5	61	18
Total,	7	4	4	9	3	7	1	1	1	1	4	4	9	26	72	355	444
Balance, April, 1868,	48	99	234	224	420	283	515	837	987	876	815	935	1000	1139	644	813	942
Received from Binders,																	
Balance, May, 1870,	41	95	230	215	417	276	514	836	986	875	811	931	991	1113	572	458	498

*See pages 301 and 302.

List of European Institutions to which Copies of Volume XVII. of the Proceedings of the American Association were distributed by the Permanent Secretary in 1869.

Stockholm, — Kongliga Svenska Vetenskaps Akademien.
Copenhagen, — Kongel. danske Vidensk. Selskab.
Moscow, — Société Impériale des Naturalistes.
St. Petersburg, — Académie Impériale des Sciences.
" " Kais. Russ. Mineralogische Gesellschaft.
" " Observatoire Physique Centrale de Russie.
Pulkowa, — Observatoire Imperiale.
Amsterdam, — Académie Royale des Sciences.
" Genootschap Natura Artis Magistra.
" Zoological Garden.
Haarlem, — Hollandsche Maatschappij der Wettenschappen.
Leyden, — Musée d'Histoire Naturelle.
Altenburg, — Naturforschende Gesellschaft.
Berlin, — K. P. Akademie der Wissenschaften.
" Gesellschaft für Erdkunde.
Bonn, — Naturhist. Verein der Preussisch. Rheinlandes, &c.
Breslau, — K. L. C. Akademie der Naturforscher.
Dresden, — K. L. C. Deutsche Akademie der Naturforscher.
Franckfurt, — Senckenbergische Naturforschende Gesellschaft.
Freiburg, — Königlich-Sächsische Bergakademie.
Göttingen, — Königl. Gesellschaft der Wissenschaften.
Hamburg, — Naturwissenschaftlicher Verein.
Hannover, — Die Naturhistorische Gesellschaft.
Leipsic, — Königlich Sächsische Gesellschaft der Wissenschaften.
Munich, — K. B. Akademie der Wissenschaften.
Prag, — K. Böhm. Gesellschaft der Wissenschaften.
Stuttgart, — Verein für Vaterländische Naturkunde.
Vienna, — K. Akademie der Wissenschaften.
" K. K. Geographischen Gesellschaft.
" Geologischen Reichsanstalt.
Württemburg, — Der Verein für Vaterländische Naturkunde.
Basel, — Naturforschende Gesellschaft.
Bern, — Allgemeine Schweizerische Gesellschaft.
" Naturforschende Gesellschaft.

Genève,—Société de Physique et d'Histoire Naturelle.
Neuchatel,—Société des Sciences Naturelles.
Bruxelles,—Académie Royale des Sciences, &c.
Cherbourg,—Société Académique.
Dijon,—Académie des Sciences, &c.
Liège,—Société Royale des Sciences. [Arts.
Lille,—Société Nationale des Sciences, de l'Agriculture, et des
Montpellier,—Académic des Sciences et Lettres.*
Paris,—Institut de France.
" Société Philomatique.
" Société Météorologique de France.
Turin,—Accademia Reale delle Scienzie.
Madrid,—Real Academia de Ciencias.
Cambridge,—Cambridge Philosophical Society.
Dublin,—Royal Irish Academy.
Edinburgh,—Royal Society.
London,—Board of Admiralty.
" East India Company.
" Museum of Practical Geology.
" Royal Society.
" Royal Astronomical Society.
" Royal Geographical Society.
Manchester,—Literary and Philosophical Society.
Batavia,—Societé des Arts et des Sciences.

* Also Volumes XV. and XVI.

REPORTS.

Professor John Torrey, from the committee on Weights and Measures, reported and recommended the following resolution, which was adopted:

Resolved, That this Association cordially approves the proposed adaptation of American coinage to the metric system, by making the value of the dollar precisely that of one and a half grains of fine gold:—seeing in this a new step toward the promotion of fraternity among nations, by the unification of weights, measures, and coinage, inasmuch as all monetary units which have simple relations to the grains must have simple relations to each other.

Prof. L. Agassiz, from the committee on the Jubilæum of Ehrenberg, reported that the duties assigned to the committee were completed. The report was accepted and the committee discharged.

Dr. B. A. Gould, from the committee on the star *Eta Argus*, reported that the duties assigned to this committee were completed, and the committee was discharged.

REPORT ON THE MICROSCOPES AND MICROSCOPICAL APPARATUS, EXHIBITED AT THE MEETING OF THE AMERICAN ASSOCIATION FOR THE ADVANCEMENT OF SCIENCE, AT SALEM, MASS., AUGUST, 1869.

THE local committee having determined to provide suitable rooms for an exhibition of Microscopes at the Salem meeting of the American Association for the Advancement of Science, a sub-committee was appointed to take charge of the exhibition. The committee having organized by the choice of Mr. James Kimball for chairman, and Mr. Edwin Bicknell as secretary, it was voted, "That the secretary prepare a suitable circular to be sent to all microscopists and others interested in the use of the microscope;" and in obedience to the vote of the sub-committee, the following circular was prepared.

A. A. A. S.

Special Circular to Persons Interested in the Use of the Microscope.—In making arrangements for the meeting of the American Association for the Advancement of Science, to be held at Salem, commencing at 10 A. M., August 18th, 1869, the Local Committee, in order to give encouragement to the general and increasing interest in the use of the Microscope, have decided to furnish rooms for the display and comparison of Microscopes, Objectives, Accessory Apparatus of all kinds, Test Objects, and Objects of Scientific and Popular interest.

It is intended to have as complete a collection as possible of instruments of both American and Foreign Manufacture. Those who are possessed of Microscope stands, Objectives, or Accessory Apparatus in any way remarkable for excellence of performance or design, are requested to bring them to the meeting.

The objects of this exhibition will be to assist the progress of scientific research, by social intercourse and a full comparison and discussion of whatever is new and important in microscopical investigation, and to encourage the manufacture and use of this valuable instrument.

Arrangements have been made to give ample opportunity for the use of the Microscopes both day and evening. A safe place has been secured for the deposit of instruments sent beforehand to the care of Mr. Bicknell, or brought by visitors who do not wish to keep them in their own possession.

A sub-committee has been appointed by the local committee to make the necessary arrangements. Farther information relating to the subject can be had by addressing

EDWIN BICKNELL,
Sec'y of the Sub-committee,
PEABODY ACADEMY OF SCIENCE.
SALEM, MASS.

SALEM, May 29, 1869.

This circular was sent, with the circular of the local committee, to the members of the Association, and to all others interested in the use of the microscope, as far as we were able to ascertain their addresses.

The distribution of this circular resulted in a gathering of twenty-five or more microscopes of different classes, including four quite old, and interesting on that account, and two not quite as old, the rest being of modern construction. Before giving a description of the modern microscopes, I shall briefly describe the old ones, and would here state that the old microscopes were shown in comparison with the modern ones in order to give the beholder a nearly correct idea of the improvements which have been made from time to time in the construction of microscopes, and to show the wonderful progress which has been made, particularly in objectives.

The oldest microscope is after the pattern described in 1694, by Hartsoeker, a Dutchman, but there is no certainty that this one was made by him. It is about two and a half inches long, and is held up to the eye to view the object, and has a condensing lens in the end opposite the eye. The object is placed between the eye lens and the condensing lens, by which it is adapted only for transparent objects. It is provided with six eye-glasses.

The next is a microscope made by G. F. Brander, in Augsburg, Germany, and is described in 1769. It is on a stand, by which it can be used either vertically, inclined, or in a horizontal position; it can be used as a simple or compound microscope, and with transparent or opaque objects. It has eight object-glasses, two of them provided with Lieberkuhns, and has a compound body, which, being removed, converts it into a simple microscope, similar to the one first described. It has a Ramsden's eye-piece mounted in wood.

The above mentioned microscopes belong to Dr. William Wood of Portland, Maine.

The next is a Lieuwenhoek microscope made by G. Adams of London, about 1750, bought by D. Van der Weyde, A. M., in Amsterdam, in 1763. It is now owned by P. H. Van der Weyde of New York. In this microscope there are six eye-glasses set in a revolving plate, so that each eye-glass can be brought in succession over the object, thus saving the trouble of unscrewing.

The next is a compound microscope, made by Charles Lincoln, 62 Leadenhall street, London, in 1770, and is owned by Dr. G. A. Perkins, of Salem. This is vertical, and stands on three scrolls and has eight object glasses with one eye-piece of the Ramsden pattern.

This concludes the notice of the old instruments.

The remaining instruments were of modern construction.

The next is by Clarke of London, is owned by the Essex Institute, Salem, was made about 1830, and was a complete instrument in its day.

The next is Andrew Pritchard's large microscope, made about 1840, also owned by the Essex Institute, and was the best of its period, having a full set of objectives and accessories.

Prof. Edward W. Morley of the Western Reserve College, exhibited a large and beautiful stand made by Thomas Ross. This has three eye-pieces, a Troughton & Sims' cobweb micrometer and goniometer, and other accessories.

Dr. D. H. Briggs of Norton, Mass., exhibited a first class Smith & Beck binocular with a complete series of objectives and accessory apparatus.

Benjamin Webb, Jr., of Salem, Mass., exhibited a first class Smith & Beck binocular with a complete outfit of objectives from three inch to one-fifteenth, by Smith & Beck, Spencer, and Wales; also a complete set of accessory apparatus.

Prof. J. Baker Edwards of Montreal, Canada, exhibited a first class binocular made by Pillischer of London, with a complete outfit of accessory apparatus and objectives by Smith & Beck and Andrew Ross. Prof. Edwards also exhibited a Sorby & Browning micro-spectroscope.

Dr. R. H. Ward of Troy, New York, exhibited a first class binocular by Crouch of London, with "Collins' graduating diaphragm," and "Crouch's universal parabolic illuminator."

Prof. B. Silliman of New Haven, exhibited a first class microscope by J. Grunow of New York, with goniometer eyepiece and objectives by Grunow, Wales, and Tolles.

Dr. P. H. Van der Weyde of New York city, exhibited a microscope made by Andrew Ross, which had a very ingenious arrangement of his own for converting it into an inverted microscope for chemical use.

Mr. Charles Stodder of Boston, Mass., exhibited a first class microscope made by the Boston Optical Works, which was a very fine instrument. It had a very thin stage which revolved on the optical centre of the instrument, making it very useful for certain purposes.

Mr. Alpheus Hyatt of Salem, Mass., exhibited a first class microscope made by Joseph Zentmayer of Philadelphia. With this was exhibited a full assortment of accessory apparatus of Mr. Zentmayer's make.

Mr. Edwin Bicknell also exhibited a first class microscope by Zentmayer, objectives by Tolles and Wales. Tolles' solid *D.* and *E.* eye-pieces, Tolles' amplifier, and one-fourth inch objective with Tolles' new illuminator for opaque objects.

A very large, and in its time complete microscope, made by Chevalier of Paris, was exhibited by Mr. Chamberlain of Boston, Mass. In addition to the objectives furnished by Chevalier, it had objectives by Andrew Ross, and Andrew Pritchard.

Mr. E. Bicknell exhibited a Zentmayer's "Army Hospital Microscope." This is admirably adapted to the use of the physician and student.

Mr. Charles Stodder exhibited four student's microscopes from the Boston Optical Works, the stands of the same pattern, but with different kinds of adjustment for focus, and different stage arrangements. Mr. Stodder also exhibited the new "Clinical" and "Seaside" microscope, of the pattern recommended by Dr. Cutter of Boston. This can be transformed into a fine pocket telescope by having a suitable object-glass adapted to it.

Prof. A. M. Edwards of New York, exhibited a student's microscope made by F. Miller & Brother of New York. This has one eye-piece and a dividing objective, thus giving two powers, and is furnished at a low cost.

Mr. F. G. Sanborn of Boston, exhibited a Murray & Heath sea-side microscope, which was a very neat instrument.

Mr. C. G. Bush of Boston, exhibited a Smith & Beck students microscope, with Smith & Beck's and Tolles' objectives.

Mr. A. H. Tuttle of Platteville, Wisconsin, exhibited a neat stand for a "dissecting microscope," in which the stand and arm-rests are made to fold into a very small compass.

Several immerson objectives were exhibited, including a $\frac{1}{15}$, $\frac{1}{10}$, $\frac{1}{6}$, $\frac{1}{4}$, by Tolles, two $\frac{1}{15}$ by Wales, and No. 10 and No. 12 by Hartnack of Paris. Mr. Tolles has recently constructed some immersion objectives for the use of the physician, for the examination of blood, pus, sputum, urinary deposits, &c. In these objectives the substance to be examined is placed upon the front lens of the objective and covered with a piece of thin glass, which is kept in place by capillary attraction. The focus is obtained by the use of the "cover adjustment." This objective applied to the "clinical microscope," before described, is a very useful instrument for the medical practitioner.

Mr. Tolles has also recently made several objectives of long focal distance on the immersion principle, which are admirably adapted to viewing objects in aquaria, or for dissecting under water. One shown by Mr. Bicknell had a power equal to an ordinary objective of $\frac{3}{4}$ inch focal distance, and with a working distance of an inch. This objective showed well the circulation of cell contents in the Nitella.

Two objectives made by Mr. Tolles of $\frac{4}{10}$ and $\frac{1}{4}$ inch respectively, were provided with his "illuminator for opaque objects," which is a glass prism set in the mounting of the objective between the front and middle combinations, so that the light is thrown down through the front lens upon the object, and so far as tested they have worked very well indeed, showing the object without glare or fog and with plenty of illumination; the $\frac{1}{4}$ being constructed upon the immersion principle, could be used upon a covered object with perfect results.

Mr. E. Bicknell exhibited a very neat apparatus for cutting sections of wood, &c.; also a new graduating diaphragm, both made by Mr. Zentmayer of Philadelphia.

EDWIN BICKNELL,

Secretary of Sub-section of Microscopy.

INDEX.

ERRATUM.

On page 45, line 15, for *it has before been shown*, read *it follows from what has before been shown.*

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